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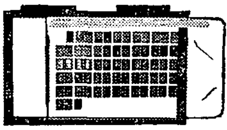

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SOCIAL DETERMINISM AND RATIONALITY AS BASES OF PARTY IDENTIFICATION*

ARTHUR S. GOLDBERG
University of Rochester

In *The Responsible Electorate,* V. O. Key urged upon us "the perverse and unorthodox argument . . . that voters are not fools."[1] He challenged the notion that the voting act is the deterministic resultant of psychological and sociological vectors. He believed that the evidence supported the view of the voter as a reasonably rational fellow. The present article offers a corollary to Key's "unorthodox argument." It suggests that certain sociological determinants, secifically group norms regarding party identification, may, upon examination, prove to be rational guides to action. For the voter who is a reasonably rational fellow, it will be argued, these group norms may seem rather sensible.

I. THE CONCEPT OF RATIONALITY

Before proceeding to the analysis of data, some discussion of the notion of rationality seems in order. The usage subscribed to in the present analysis derives from contemporary game theory.[2] Put most simply, being rational in a decision situation consists in examining the alternatives with which one is confronted, estimating and evaluating the likely consequences of each, and selecting that alternative which yields the most attractive set of expectations. Formally, this process entails making calculations of the following type as a basis for the decision:

$$\begin{aligned} E(Va_1) &= P(o_1|a_1)V(o_1) \\ &\quad + P(o_2|a_1)V(o_2) + P(o_3|a_1)V(o_3) \\ E(Va_2) &= P(o_1|a_2)V(o_1) \\ &\quad + P(o_2|a_2)V(o_2) + P(o_3|a_2)V(o_3) \end{aligned}$$

where:

$E(Va_i)$ = expected value of alternative i.
$P(o_j|a_i)$ = probability of outcome j given that alternative i is chosen.
$V(o_j)$ = value of outcome j to the decision maker.

Having made such calculations, one then chooses the alternative with the greatest expected value. All individuals are assumed to act so as to *try* to maximize expected value. In this sense, all are equally rational. However, there is room for error in these calculations. Individuals may, therefore, vary in their effective rationality.

There are several sources of error. The relevant probabilities are not necessarily known, and may have to be estimated; the quality and cost of information thus affect the accuracy of the calculations. The calculations themselves are not cost free, being far more costly for some than for others. However, one component of these calculations is not admissible within the theory as a source of error, i.e., the values assigned to outcomes. The placing by an actor of a very high value for him upon having a Catholic for President, or upon obtaining an outcome which will aggravate his brother-in-law, or upon thwarting some target group cannot be adjudged within the theory. Therefore, in the analysis

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* Much of the development and analysis in this article is a byproduct of efforts to develop a dynamic model of voting behavior. These efforts were funded by the National Science Foundation during the summers of 1966 and 1967. The body of data used in the present analysis was provided by the Survey Research Center at the University of Michigan. It is comprised of the 1956 election study (#417) plus occasional items from the 1958 and 1960 waves of the same panel.

[1] V. O. Key, Jr. *The Responsible Electorate* (Cambridge, Massachusetts: The Belknap Press of Harvard University Press, 1966), p. 7.

[2] See R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley and Sons, Inc., 1957), Chapter II.

which follows, terms such as "more rational" and "less rational" refer in all cases to effective rationality, i.e., to the accuracy of the expected value calculations. No set of values is being judged against some exogenous criterion of "rational correctness."

However, the analysis is heavily dependent upon an assumption about effective rationality. Specifically, the assumption is made that, *ceteris paribus,* effective rationality, i.e., the accuracy of one's expected value calculations, increases as a function of education. Education is assumed to lower information costs, e.g., by improving one's ability to read and comprehend such information sources as *The New York Times,* and thus to improve the accuracy of one's estimates of relevant probabilities. Education is also assumed to act upon innate intelligence so as to develop it toward its full potential. Thus, errors in the performance of the calculations ought to be reduced. In brief, education is assumed to facilitate accurate perceptions of means-ends relationships and to facilitate appreciation of the import of such relationships for one's own goals.[3]

Of late, there has been emerging from the voting literature a model of the voter as rational maximizer of expected utility.[4] The analysis of empirical evidence in the context of this model is still quite rare, but by no means non-existent. Current intellectual investments in the model, and the very nature of its logical qualities suggest that it will increasingly become a central concept in the discipline.[5] In its application, the political scientist is, of course, primarily interested in the substance of the expected value calculations. One is particularly concerned with ascertaining which factors are heavily weighted in the calculations. Are they policy outputs and promises as in the Downs model,[6] non-policy based anxieties as in some psychological findings,[7] desires for symbolic gratifications,[8] or what? What is the distribution of dominant factors across the electorate? Key was persuaded that policy counted heavily. (In fact, he seemed willing to regard behavior as rational only where this was the case—a usage different from that in the present analysis.) The present article, while not directly addressed to this question, is substantially related to it. The question at issue here is whether group political norms are seen as effective means to their desired ends by those best equipped to make the judgment.

II. A CONFLICT OF MODELS?

One of the most powerful determinants of voting behavior isolated to date appears to be the self-identification of the voter with a particular party. Empirical findings to this effect are legion.[9] Moreover, there is a substantial empirically-based literature, which indicates that an individual develops a party identification quite early in life, during childhood, in fact. This identification is well-established before the individual has any knowledge of either the policy history of the party with which he identifies, or of the implications of that history for his own interests. Essentially, the acquisition process appears to consist in imitative and affective conditioning to the parental partisan preference.[10] Once acquired the identification proves to be remarkably stable, with some 70–80% of those voters who know their paternal party identifications reporting the same identifications for themselves.[11]

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[3] One may regard education, from this point of view, as a capital investment oriented toward long-run reduction of information and calculation costs. For these notions on rationality and education, I am indebted to Peter C. Ordeshook, from whose work on a general rationality model of electoral behavior they are derived.

[4] The seminal work here has been Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957). For critical appraisals see: Donald E. Stokes, "Spatial Models of Party Competition," this *Review,* 57 (1963), 368–377; and Philip E. Converse, "The Problem of Party-Distances in Models of Voting Change," in M. Kent Jennings and L. Harmon Zeigler (eds.), *The Electoral Process* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1966), Chapter IX.

[5] See, for example: James M. Buchanan and Gordon M. Tullock, *The Calculus of Consent* (Ann Arbor: The University of Michigan Press, 1962); Otto A. Davis and Melvin Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," *Mathematical Applications in Political Science, II.* Arnold Foundation Monographs XVI. (Dallas, Texas: Arnold Foundation, Southern Methodist University, 1966), 175–208; and William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," this *Review,* 62 (March, 1968), 25–42.

[6] Downs, *op. cit.*

[7] For example, in Eugene Burdick and Arthur J. Brodbeck (eds.), *American Voting Behavior* (Glencoe, Illinois: The Free Press, 1959), see Arthur J. Brodbeck, "The Problem of Irrationality and Neuroticism Underlying Political Choice," Franz Alexander, "Emotional Factors in Voting Behavior," and Richard E. Renneker, "Some Psychodynamic Aspects of Voting Behavior."

[8] See Murray Edelman, *The Symbolic Uses of Politics* (Urbana, Illinois: University of Illinois Press, 1964).

[9] See, for example: Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (The University of Chicago Press, 1954), Chapters IX–X; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, "Party Loyalty and the Likelihood of Deviating Elections," *Journal of Politics,* 24 (1962), 689–702.

How well does the notion of the rational voter comport with the evidence cited which suggests that a party identification, established in childhood and seemingly quite resistant to change, is a major determinant of voting behavior? Having no wish to build straw men, let me immediately point out that there is not necessarily an incompatibility here. If one admits the possibility that the imitatively and affectively conditioned party identification learned in childhood might prove a rationally sound guide to action when examined in adulthood, a possible basis for reconciliation is available. This is the possibility which will be explored in the present analysis.

While the evidence to be examined sheds light on the question at hand, some of that evidence was initially analyzed for a different purpose. An appreciation of the meaning of the evidence requires that a somewhat circuitous route be taken in examining it. Some forebearance is therefore sought from the reader in the initial stages of the analysis. In return, assurance is given that the initial data presented, while somewhat removed from the main question, are themselves of substantive import, and lead to a sharp focus upon the main question, namely, rationality and social determinism as bases of party identification.

III. BACKGROUND OF THE INVESTIGATION

The original topic of inquiry was defection from parental party identification. In seeking to account for this relatively infrequent phenomenon, some unanticipated findings were encountered. After a bit of retroductively oriented contemplation, it became possible to "make sense" out of these findings. Quite simply, they made sense if the norms proffered by the parent were substantively rational for the self-interest of the offspring, i.e., increased his effective rationality. Since an explanation can be found for almost any set of findings, the discovery of an explanation was not, in itself, taken as proof of the validity of the explanation. Instead, several deductions were made, based upon the foundations of this explanation. Upon investigation, these deductions were found to be supported by the data, and, as a result, the reader is burdened with the present article. At the outset one must dwell a bit upon the original inquiry in order to appreciate the nature of the unanticipated findings.

Since the object of that inquiry—defection from parental party preference—occurs rather infrequently, some care was used in conceptualizing the independent variables in order that they might more readily be operationalized in productive ways. One of the major variables initially under consideration was social mobility. The usual reasoning applied: it was assumed that such mobility put the individual into a new social environment, the norms of which might be different from those of his parents. The resultant pressures of his adult environment might produce a shift on his part toward the norms of the new environment, and thus an abandonment of the parental party preference. A bit of additional thought on this matter, however, revealed that the logically implicit variable was not necessarily social mobility, but a strain between the parental partisan norm and the norm of the adult social environs. Moreover, there was an additional path to such strain, beyond that provided by social mobility. Specifically, it is logically possible for such strain to develop when the parental norms are atypical in their social environs and the individual remains in those environs in adulthood. Thus, the individual whose father is an Irish, blue-collar worker, and a Republican in an urban Democratic neighborhood and who is himself a blue-collar worker in that neighborhood, may be under as much strain as he would have been had his father been a Democrat while he himself became a physician and moved to the suburbs. It was not too difficult to devise a technique for measuring this hypothetical strain, however caused.

The technique used was to develop an "expected value" of the respondent's being a Republican, based upon his sociological characteristics.[12] In order to do this the respondent's party identification (Coded Republican = 1, Democrat = 0) was regressed upon five of his sociological characteristics in a multiple regression analysis.[13] The expected value thus generated is effectively an estimate of the probability that an individual with a given set of sociological characteristics is a Republican. One can compare this value with the father's party identification (Coded Republican = 1, Democrat = 0) and see how close the respondent's probability of being a Republican is to his father's actual state of being one. The absolute difference is a measure of strain between partisan norms of the respondent's adult environment, and the partisan preference of his father.[14] This concept will be referred to as intergenerational strain. Recapitulating: intergenerational strain is operationalized as the absolute difference between the father's party identification (Coded Republican = 1, Democrat = 0) and the respondent's regression-generated probability of being a Republican, given his sociological characteristics.

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[10] See: Herbert H. Hyman, *Political Socialization* (Glencoe, Illinois: The Free Press, 1959), Chapters II–III; David Easton and Robert D. Hess, "The Child's Political World," *Midwest Journal of Politics,* 6 (1962), 229–246; and Fred I. Greenstein, *Children and Politics* (New Haven: Yale University Press, 1965), Chapter IV. An overview of the socialization processes involved is provided in Robert E. Lane and David O. Sears, *Public Opinion* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1964).

[11] Cf. Campbell, *et. al., op. cit.* p. 147; see also Philip E. Converse and Georges Dupeux "Politicization of the Electorate in France and the United States," *Public Opinion Quarterly,* 26 (1962), p. 14.

[12] The reader should note, at this point, that in

Given this perhaps elaborate operational procedure, one may well ask, does intergenerational strain account for much defection from the parental norm? The data are presented in Table I and again in Figure 1. That the hypothesized relationship is borne out by the data is fairly obvious. Since the frequencies associated with the scores 0.0 and 0.1, as well as with the scores 0.8–1.0, are quite small, interest centers on the scores between and inclusive of 0.2 and 0.7. Here the hypothesized relationship does hold, although the relationship is "S" shaped rather than linear. Whether much of the defection is thereby explained is another question. As against the standard of 100% of the variance, clearly not much is accounted for. Even using an asymmetric measure of association, Goodman and Kruskal's tau, designed to take advantage of the expectation that there is more information in the independent variable about the dependent variable than vice versa, errors in assignment on the dependent variable are deduced only by about 5.5%.[15] On the other hand, as against other single variable accomplishments with regard to this dependent variable, 5.5% is not trivially small.[16] This finding was, in itself, neither dramatic nor surprising. However, it was in the process of exploring the origins of intergenerational strain that a rather surprising finding did emerge, which in turn proved to be an opening wedge into the inquiry with which the present article is concerned.

In the interest of neatness, if nothing else, it

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the operation to be described, statistical averages of a national sample were used as representations of felt environmental norms. This dubious procedure will be discussed at a later point in the analysis (see pp. 10, below). Particular sociological characteristics were chosen with a view toward reducing the discrepancies between such averages and felt norms and with a concern for minimizing multicollinearities. The specific characteristics utilized were: religion, self-rating on class, size of community, region (South, non-South), and race (white, non-white).

[13] In all cases, nominal data were converted to dummy variables and treated as prescribed in Daniel B. Suits, "The Use of Dummy Variables in Regression Equations," *Journal of the American Statistical Association,* 52 (1957), 548–551.

[14] The reader's attention is called to the fact that paternal partisan preference is ascertained on the basis of respondent's recall. Work by M. Kent Jennings and Richard G. Niemi suggests something of the magnitude of the error thus induced. From the point of view of correlation, Niemi finds a Kendall's tau of 0.60 between party identification claimed by the respondent's father and that ascribed to the father by the respondent. See Richard G. Niemi, *A Methodological Study of Political Socialization in the Family* (unpublished doctoral dissertation, University of Michigan, 1967), pp. 113–115, and 128. A further analysis of this data by this writer with the cooperation of Professor Niemi indicates that 78.4% of the students correctly identify their fathers in the broad categories of Democrat, Independent, Republican. The 21.6% of incorrect identifications are about evenly split between incorrect assignment with regard to the category of Independent (10.6%) and assignment to incorrect partisanship (11.0%).

Insofar as there is any systematic bias in the errors, it seems to operate in a manner not seriously debilitating to the present analysis. Respondents appear to be biased toward perceiving their parents as having a partisanship which is "normal" for people of like social characteristics. Thus where parents are identified as having a sociologically deviant identification, the ascribed identification is apt to be correct. This is rather important as such identification proves of central concern in the present analysis. See Niemi, *op. cit.*, pp. 126–128.

[15] See Hubert M. Blalock, Jr., *Social Statistics* (New York: McGraw-Hill Book Company, Inc., 1960), pp. 232–234.

[16] About the best that has been done along these lines with a single variable is the 7.29% of the variance in level of voter stability explained by family reinforcement, in Herbert McClosky and Harold E. Dahlgren, "Primary Group Influence on Party Loyalty," this REVIEW, 53 (1959), 774.

TABLE I. DEFECTION AS A FUNCTION OF INTERGENERATIONAL STRAIN[a]

	Score on Strain											
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	Total
Defection	4	1	22	51	67	46	51	17	2	—	1	262
Loyalty	7	11	109	234	135	73	55	25	2	—	2	653
Total	11	12	131	285	202	119	106	42	4	0	3	915
% Defecting $r_b = 5.5\%$	36.4	8.3	16.8	17.9	33.2	38.7	48.0	40.5	50.0	—	33.3	28.6

[a] $p < .05$ in this and other Tables, unless otherwise indicated.

seemed desirable to see how much of strain derived from social mobility on the part of the respondent, and how much of it derived from parents who had party identifications which were sociologically atypical. It was not anticipated that these deviant parental identifications would account for much strain, but some method had to be devised for measuring this "deviance" in order to put the question to rest.

Fortunately a measure of such deviance was readily at hand. The residual in a regression analysis is theoretically a measure of the extent to which an element differs in its dependent variable value from the average of other elements having the identical values on the independent variable(s). Thus, a multiple regression analysis in which the respondent's father's sociological characteristics were the independent variables and in which the father's party identification was the dependent variable would yield

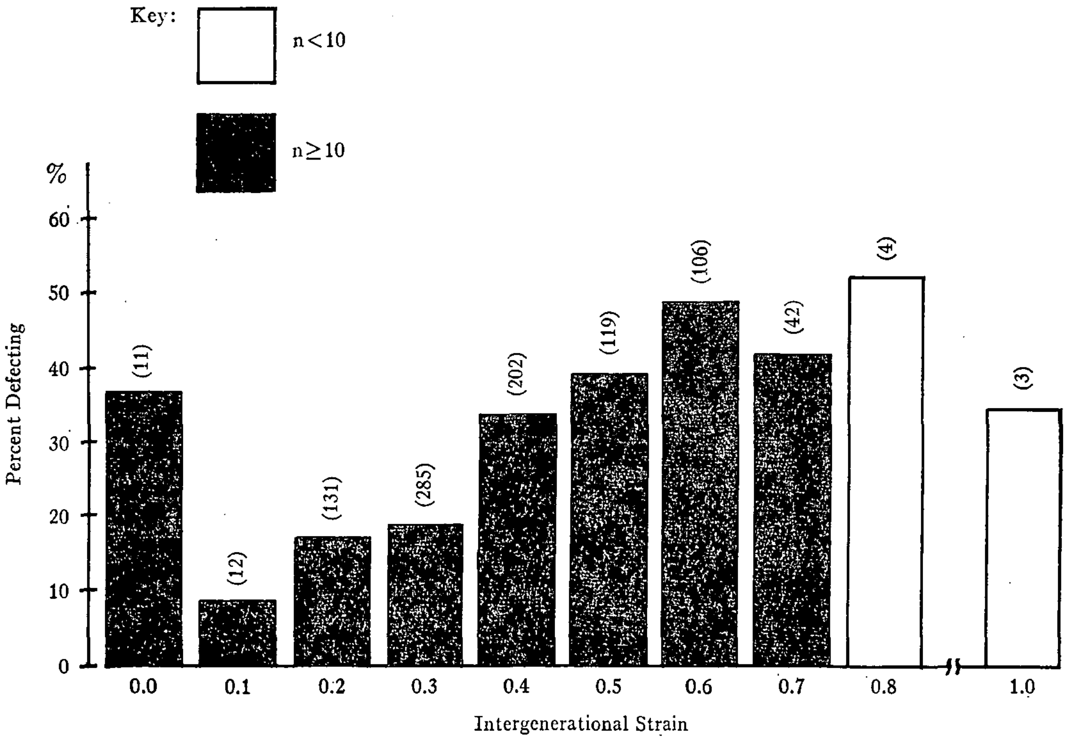

Fig. 1. Defection as a Function of Intergenerational Strain.

residuals well suited to the present task. Indeed, just such a regression was run, and the absolute values of the residuals thus generated were used to measure the extent of sociological deviance in paternal party identification.[17] It may be of some comfort to note that those parents who had the highest residuals, 0.847 and 0.718, were intuitively quite deviant, being on the one hand white, working class, Catholic, Northern, urban, Republicans, and on the other white, middle-class, Protestant, Northern, rural, Democrats. Similarly, those with the smallest residuals, 0.000 and 0.282, were what one would tend to regard as 'pure' types, being white, working class, Catholic, Southern, urban Democrats, and white, middle class, Protestant, Northern, rural Republicans. Withal, there are some caveats, beyond those concerned with statistical propriety, which should be observed. Statistical deviance from a sample norm and social deviance from a felt group norm are not identical, and yet the present study treats them as though they were. The most statistically deviant case in the national sample—the white, working-class, Catholic, Northern, urban Republican—can be found feeling not the least bit deviant and under no social pressures, in, for example, the city of New Haven, Connecticut.[18] Yet such errors of measurement, as they might affect the present study, would of necessity do so in a conservative way, biasing the outcomes *against* the findings which proved to be of interest. Thus, if such findings emerged, they did so in spite of and not because of the weakness in the operational measure.

TABLE II. INTERGENERATIONAL STRAIN AS A FUNCTION OF DEVIANCE IN PATERNAL PARTY IDENTIFICATION

	Score on Deviance[a]			
	High	Low	Total	% High Deviance
High Strain[a]	221	53	274	80.6
Low Strain	49	592	641	7.6
Total	270	645	915	29.6
% High Strain	81.9	8.2	29.9	
$\tau_b = 53.9\%$				

[a] High implies >0.500
Low implies <0.500
No respondent scored 0.500

How much, then, of intergenerational strain results from sociologically deviant paternal party identification? The r^2 between the two variables is 0.58. That is to say, 58% of the variance in one is explainable in terms of the other. Thus, there is a substantial covariation between the two. One can say more than this, however, after examining the pattern of the relationship. This pattern is revealed in Table II, wherein the variables are each dichotomized, with the attendant loss of information, and yet still yield a tau of 53.9%. Most particularly, one should note in this Table that 80.6% of high intergenerational strain seems to result from highly deviant paternal party identification.

Given so strong a relationship between intergenerational strain and sociological deviance in paternal party identification, it certainly seemed advisable to inquire into the effect of such deviance on the relationship between intergenerational strain and defection. This effect is set forth in Table III and is quite noteworthy. In the universe comprised solely of those whose paternal party identification is sociologically normal, i.e., where strain results only from social mobility, there is no significant relationship between differences in strain and rate of defection. In the universe comprised solely of those whose paternal party identification was sociologically deviant, there is a significant relationship between strain and defection. In the light of Table II one might be inclined to impute *all* of the high strain in this group to the deviant nature of the paternal party identification. However, this assumption is not altogether warranted. Therefore, some direct evidence was sought in the data regarding social mobility. Fortunately this was available in the form of information about intergenerational occupational status mobility.

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[17] Clearly some qualms arise in using the residuals of a linear regression analysis in which the dependent variable was dichotomous. One worries about saturation effects, etc. [See Arthur S. Goldberg, *Econometric Theory* (New York: John Wiley and Sons, Inc., 1964), pp. 248–251.] In the actual event, visible saturation effects were infrequent (less than 1% of the cases had predicted values less than zero or greater than one). Moreover, the dichotomous nature of the dependent variable offered certain advantages. First, there is only one possible way to obtain each non-zero value of the residual. Second, there are only two points at which the residuals can be zero, namely, where the predicted and actual values are zero, and where the predicted and actual values are one. These polar points become, in effect, the "normal" points for their respective segments of the distribution (this division occurring at that value of X which yields a predicted value of y of 0.5). Thus, by comparing any two cases on the basis of the absolute values of their residuals, one can say which is closer to his respective "normal" point. It is on this basis that sociological deviance in paternal party identification was measured.

[18] See Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), Chapter V, especially Table 5.4, p. 60.

TABLE III. DEFECTION AS A FUNCTION OF INTERGENERATIONAL STRAIN CONTROLLING FOR DEVIATION OF PATERNAL PARTY IDENTIFICATION

(a) Given normal paternal party identification:

	Score on Strain		
	High	Low	
Defection	14	130	144
Loyalty	39	462	501
	53	592	645
% Defecting	26.4	22.0	22.4
$p > 0.05$			
$r_b = 0.2\%$			

(b) Given deviant paternal party identification:

	Score on Strain		
	High	Low	
Defection	103	15	118
Loyalty	118	34	152
	221	49	270
% Defecting	46.6	30.6	43.7
$p < 0.05$			
$r_b = 1.7\%$			

Therefore, the "high strain" category of Table III (b) was examined in terms of the relationship between occupational status mobility and defection. As can be seen in Table IV, there does not appear to be any significant relationship. Thus, on the bases of Tables III and IV, social mobility does not appear to be a significant factor in explaining defection from paternal party identification.

TABLE IV. DEFECTION AS A FUNCTION OF INTERGENERATIONAL OCCUPATIONAL STATUS MOBILITY GIVEN DEVIANT PATERNAL PARTY IDENTIFICATION AND HIGH INTERGENERATIONAL STRAIN[a]

	Intergenerational Occupational Status Mobility		
	Upward	None	Downward
% Defecting (N)	48.0 (77)	43.4 (67)	49.4 (75)

[a] Two of the two hundred and twenty-one respondents in this category provided no information on occupational status mobility. The total N here is therefore 219 as against the 221 in the relevant column of Table III (b).

This finding concurs with that of Campbell, *et al.*, in their slightly different analysis of the same body of data.[19] However, it does seem to fly in the face of the current lore in the discipline. For example, Henry W. Riecken, in a survey of the literature dealing with primary groups and party affiliation, argues that:

> Upward mobility . . . signals not only alienation from parental values, but the likelihood that political preference will shift from Democratic to Republican since the upward mobile person will be moving into predominantly Republican affiliations in primary groups.[20]

He goes on to cite the works of Berelson, Lazarsfeld, and McPhee, of Maccoby, *et al.*, and of McClosky and Dahlgren in evidence.[21]

Surely such apparently conflicting conclusions about the role of social mobility in accounting for intergenerational changes in party identification deserve serious treatment. However, since the material to this point is very much by way of preface, such as extended digression here seems ill advised. Therefore a consideration of the works cited, as they bear upon this problem, is provided as an appendix to the present article. At this point, let it suffice to note that the assertion made in the present study, i.e., that social mobility is not a significant factor in explaining defection, is compatible with the assertion that social mobility, when it occurs, tends to produce defection. All that is required for this compatibility is that either there be relatively little social mobility or that the tendency toward defection induced by social mobility be rather weak. In the actual event, it appears that both conditions exist. For a fuller discussion, the reader is referred to the Appendix.

Presented with the substantial relationship between intergenerational strain and deviance in

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[19] Campbell, *et. al.*, *op. cit.*, pp. 458–459.

[20] Henry W. Riecken, "Primary Groups and Political Party Choice," in Burdick and Brodbeck, *op. cit.*, p. 167.

[21] *Ibid.*, pp. 167–168. The relevant works are: Berelson, Lazarsfeld, and McPhee, *op. cit.;* Eleanor E. Maccoby, Richard E. Matthews, and Anton S. Morton, "Youth and Political Change," *Public Opinion Quarterly*, 18 (195?), 23–29; McClosky and Dahlgren, *op. cit.* An intimation of the findings of the present study may be seen in Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in David E. Apter (ed.), *Ideology and Discontent* (London: Collier-Macmillan Limited, The Free Press of Glencoe, 1964), pp. 231–233.

TABLE V. DEFECTION AS A FUNCTION OF DEVIANCE OF PATERNAL PARTY IDENTIFICATION

	Score on Deviance											
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	Total
Defection	6	17	48	48	25	36	40	35	5	1	1	262
Loyalty	3	67	198	158	75	39	58	40	14	1	0	653
Total	9	84	246	206	100	75	98	75	19	2	1	915
% Defection $\tau_b = 6.4\%$	66.7	20.2	19.5	23.3	25.0	48.0	40.6	46.6	26.2	50.0	100.0	28.6

paternal party identification, as well as with the vitiating effect of such deviance when used as a control on the original relationship between strain and defection, one was naturally curious as to the direct relationship between sociological deviance in paternal party identification and defection from paternal party identification on the part of the offspring. The data are presented in Table V and in Fig. 2. The relationship is somewhat stronger than the original one between intergenerational strain and defection (τ_b = 6.4% in the present case as against 5.5% in the earlier case). However, interest in the relationship derives not from its magnitude which

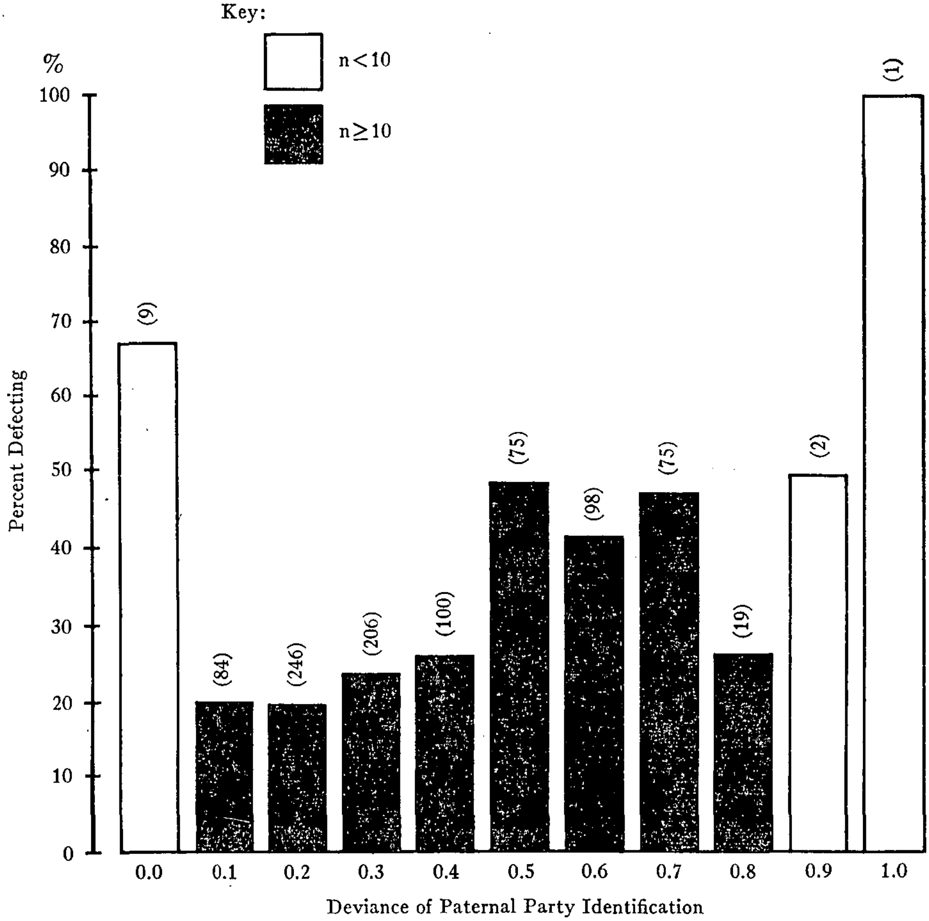

FIG. 2. Defection as a Function of Deviance of Paternal Party Identification.

is, after all, still quite modest, but from the fact that this phenomenon of sociologically deviant paternal party identification appears to be a more important source of intergenerational defection than does social mobility on the part of the offspring. Sociological deviance in paternal party identification thus assumes a heretofore unanticipated importance, and is perhaps deserving of serious recognition in future analyses.[22]

In the present analysis, this new variable, however intrinsically interesting, does not of itself shed light on the central problem of concern. This, the reader may recall, is the potential conflict between the rational voter model and the evidence for social determinism in the establishment of party identification. That sociological deviance in paternal party identification would produce defection among offspring who remained in the same social environs was anticipated under the general social determinism model: only the relatively high incidence of such cases was unanticipated. However, when a relatively routine control was applied to the relationship between deviant paternal party identification and defection, several findings emerged which appeared quite germane to the central concern of this inquiry.

IV. THE PRECIPITANT OF THE PRESENT INQUIRY

Pursuant to relatively standard operating procedure, respondent's education was applied as a control to the relationship between deviance in paternal party identification and defection from that identification on the part of the respondent. There was, at that point in the analysis, no genuine theoretical expectation as to the effect of this control. It did, however, produce a rather unexpected finding. The control revealed that the better educated respondents were more likely to defect from deviant paternal party identification than were the less well educated, and, conversely, the better educated respondents were less likely than the poorly educated to defect from normal paternal party identification.

As indicated earlier in Table V, errors in assignment on the dependent variable (defection from paternal party identification) are reduced by 6.4% by a knowledge of the extent to which paternal party identification was sociologically deviant.[23] When one controls for education (dichotomizing the sample at the median), the τ_b is increased to 11.3% in the universe of the better educated, whereas, in the universe of the poorly educated it is reduced to 3.4%. (These results may be seen in the data presented in Table VI, and in Fig. 3.) Education appears to sensitize one to the fact of deviance in paternal party identification where such deviance is or has been present. Moreover, where the father has been normally identified, education of the offspring enhances the likelihood of filial loyalty. Perhaps this may

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[22] At this point, it may be worth noting that this concept (sociological deviance) helps to clarify at least one peculiar finding in the existing literature. In *The American Voter,* Campbell, *et al.,* after noting that the direction of intergenerational social mobility, as measured by occupational status differences, did not seem to be associated with shifts toward the Republican or Democratic identification go on to note, on pp. 458–459, n. 15, that:

> Upward mobile people are slightly more likely to have shifted from Democrat to Republican identification, than those people whose status has moved downward, *but both types of changers* are more likely to have moved toward the Republican Party than away from it. [My italics.]

However, this appears to be a consequence of the operation of sociological deviance in paternal party identification, because when analysis is confined to those respondents whose fathers were normally identified, the findings are more in line with what one might traditionally have expected. Upward mobile offspring of Democrats are more apt to become Republicans than are downward mobile offspring of Democrats. Conversely, downward mobile offspring of Republicans are more apt to become Democrats than are upward mobile offspring of Republicans. Moreover, contrary to the SRC suggestion, the downward mobile progeny of normal Republicans are more likely to become Democrats than are such progeny of normal Democrats to become Republicans. Finally, where occupational status has not changed between generations, the Republicans have lost more than the Democrats—a finding compatible with the history of the Republican party over the past thirty years.

DEFECTION AS A FUNCTION OF OCCUPATIONAL STATUS MOBILITY: GIVEN NORMAL PATERNAL PARTY IDENTIFICATION

	Upward	None	Downward
Father Democrat			
% Defecting	26.4	12.3	18.0
N	(167)	(114)	(122)
Father Republican			
% Defecting	24.5	22.2	29.0
N	(94)	(72)	(76)

[23] Strictly speaking, the reduction is calculated against a null model which is based upon random assignment in proportions prescribed by the distribution on the dependent variable. For clarification, the reader is again referred to Blalock, *loc. cit.*

TABLE VI. DEFECTION AS A FUNCTION OF DEVIANCE OF PATERNAL PARTY IDENTIFICATION, CONTROLLING FOR EDUCATION[a]

(a) Given high education level on the part of the respondent:

	Score on Deviance											
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	Total
Defection	3	12	27	22	13	19	24	20	1	—	1	142
Loyalty	1	42	125	100	45	16	28	16	5	—	0	378
Total	4	54	152	122	58	35	52	36	6	0	1	520
% Defecting $r_b = 11.3\%$	75.0	20.2	17.7	18.0	22.4	54.3	46.1	55.5	16.7	—	100.0	27.3

(b) Given low education level on the part of the respondent:

	Score on Deviance											
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	Total
Defection	3	5	21	26	12	17	16	15	4	1	—	120
Loyalty	2	25	73	58	30	23	30	24	9	1	—	275
Total	5	30	94	84	42	40	46	39	13	2	0	395
% Defecting $r_b = 3.4\%$	60.0	16.7	22.4	31.0	28.6	42.5	34.8	38.5	30.8	50.0	—	30.4

[a] Dichotomization of education level:
low = none through some high school; high = more than some high school

be more clearly seen in the percentages in Table VII, despite the loss of information entailed in dichotomization of the independent variable. Most likely to defect are the well educated offspring of fathers with sociologically deviant party identifications; the defection rate here being 50.0%. Least likely to defect (19.7%) are the well educated offspring of normally identified fathers. Among poorly educated respondents, those whose paternal party identifications were deviant are more likely to defect than are those whose paternal party identifications were normal (37.9% as against 26.2%). However, poorly educated respondents whose paternal party identifications were deviant, are less likely to defect than are well educated respondents with similar paternal antecedents (37.9% as against 50.0%). Finally, the poorly educated offspring of normally identified fathers are more likely to defect than the well educated offspring of normally identified fathers (26.2% as against 19.7%).

TABLE VII. DEFECTION AS A FUNCTION OF DEVIANCE IN PATERNAL PARTY IDENTIFICATION CONTROLLING FOR RESPONDENT'S LEVEL OF EDUCATION

Percent Defecting
($N = 915$)

		Education High	Education Low
Paternal Party Identification[a]	Deviant	50.0 (130)	37.9 (140)
	Normal	19.7 (390)	26.2 (255)

[a] Deviant: abs value of residual > 0.5
(see above p. 9)
Normal: abs value of residual < 0.5

None of these differences in percent defecting are very likely to be due to chance. Of the four comparisons made, and on the basis of two-tailed tests, only one has a probability of

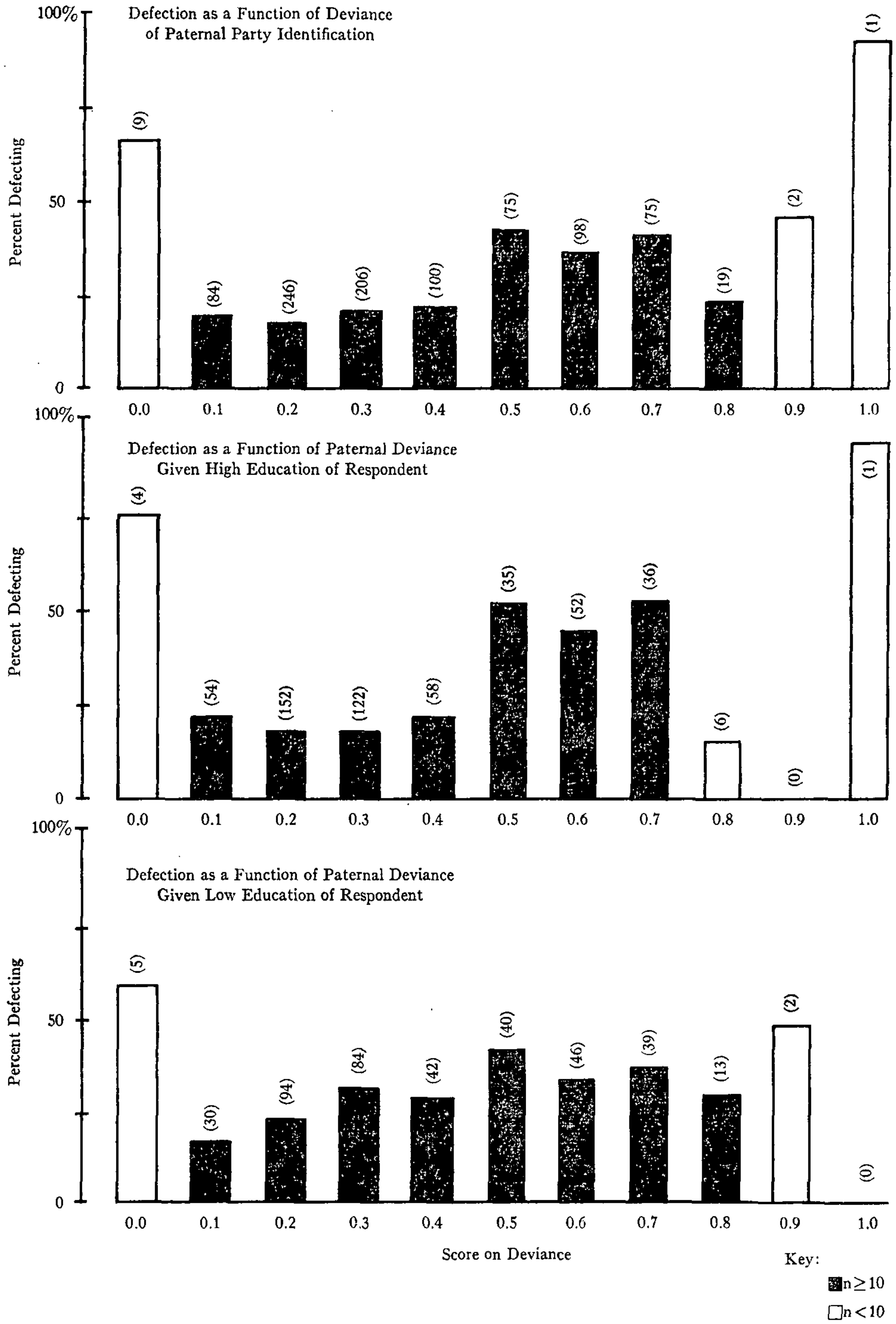

Fig. 3. Impact of Education on Defection as a Function of Paternal Deviance.

occurrence by chance greater than 0.05. The difference in question is that between the defection rates of poorly educated as against well educated respondents with normally identified fathers (26.2% as against 19.7%). Even here the probability of occurrence by chance is only 0.0525. These findings, then, seem to warrant serious attention.[24] Assuming for the moment that they are not a function of measurement error—a problem which will be discussed later—under what theoretical scheme do they make sense? Or put somewhat differently, under what set of assumptions would one expect these results?

V. EXPLANATIONS

There are at least two sets of assumptions under which one would expect these results. One of these, the "brainwashing" model, entails only one assumption, namely, that the educational system of the country "brainwashes" people into believing in their group norms. This model will not be treated seriously in the present study. It is mentioned because it is a logical alternative to the model which will be offered, and because none of the tests performed on the latter in this study are capable of discriminating between the two models. Thus, should any reader wish to dwell upon the matter to the extent of conceiving of a discriminating test, a service would be rendered.

The model which will be treated seriously, the "rationality" model, consists of the general utility maximization model plus three assumptions. These are: 1) that group-based norms regarding party identification produce effective rationality with regard to the goals of the group; 2) that the individual in the group shares the goals of the group; 3) that education increases rationality in the special senses of lowering information costs and developing innate intelligence toward its full potential.[25] Under this set of assumptions, one would expect the well educated respondents to be better able than the poorly educated respondents to accurately judge the effectiveness of the parental party identification as a guide for the implementation of their own goals. One would also expect, from this set of assumptions, that the sociologically normal identification would be more efficacious than deviant identifications for the accomplishment of the respondents' goals. The well educated being more readily able to see this than the poorly educated, one would expect defection rates in the following rank order, from highest to lowest: paternal party identification sociologically deviant and respondent well educated; paternal party identification deviant and respondent poorly educated; paternal party identification normal and respondent poorly educated; paternal party identification normal and respondent well educated. Thus, these assumptions lead to a set of expectations congruent with the empirical findings; i.e., they provide an explanation for the findings. Of course, an explanation's discovery is not to be taken as proof of its validity. Therefore, attention is shifted to testing, by way of empirical evidence, deductions derived from these explanatory assumptions.

VI. EDUCATION, DEFECTION AND RATIONALITY

Before proceeding with a description of these deductions, their testing, and the results thereof, a few words are very much in order regarding the direct impact of education in intergenerational defection. Insofar as there was, prior to the explanatory assumptions suggested above, any expectation with regard to this impact, it was that probability of defection would increase with respondent's level of education. This expectation was derived from the work of Maccoby, Mathews, and Morton who found that in Cambridge, Massachusetts, education was positively correlated with defection.[26] Upon application of Goodman and Kruskal's tau to their findings, one finds that a knowledge of ed-

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[24] Lest the reader be apprehensive that the findings in Table VI reflect *only* the impact of education on offspring of sociologically deviant Democratic identifiers, through the ensuring that such offspring will be in a Republican environment, a further breakdown is presented below.

DEFECTION AS A FUNCTION OF DEVIANCE IN PATERNAL PARTY IDENTIFICATION, CONTROLLING FOR RESPONDENT'S LEVEL OF EDUCATION: BY PARTISANSHIP OF FATHER

		Percent Defecting			
		Father Democrat		Father Republican	
		High Edu.	Low Edu.	High Edu.	Low Edu.
Paternal Party Identification	Deviant	56.8 (74)	38.7 (62)	41.1 (56)	37.2 (78)
	Normal	18.7 (235)	23.5 (170)	21.2 (155)	31.8 (85)

Two points are particularly worth noting in this breakdown. First, the pattern found in Table VII holds, regardless of paternal party preference. Second, normally identified Democratic parents also help to insure the political loyality of their children by furthering their educations.

[25] See n. 3, above.

[26] Maccoby, Matthews, and Morton, *op. cit.*

ucation yields a reduction in assignment errors on defection of 4.5%. Thus a similar relationship was expected in the present body of data. However, this did not prove to be the case; a very weak relationship was found, which did not prove significant at the .05 level. The data are presented in Table VIII.

The discrepancy between the findings of the Maccoby *et al.* study and the present one may reflect differences in sample bases or in operational procedures. Direct comparisons between the two studies are not quite legitimate, since the operationalization of the dependent variable, defection, was accomplished in substantially different ways. This operation in the present study entails only the establishment of whether or not the respondent continues to identify himself with the party with which he believes his father to have associated himself. Maccoby *et al.*, on the other hand, have built an index which takes into account the respondent's party identification and current vote preference, and the relationship of these preferences to those of both of his parents.[27]

More important for present purposes than their empirical finding, is the rationale which Maccoby *et al.*, offer for it.

> Presumably, in the course of obtaining an education, the young person is exposed to a wider variety of points of view and more information, so that he is able to make political choices on the basis of "rational" considerations rather than simply on the basis of family tradition (or revolt against family tradition).[28]

Although there is some ambiguity in their explanation—exposure to more points of view and more information could lead to increased defection, even without any increase in the rationality of political choice—the idea that education might produce more rational choice is certainly intriguing, as well as compatible with the assumptions offered in the present study. However, to argue that an increase in rationality—however one wishes to define it—must produce an increase in defection from the traditional primary group party identification is to argue that the latter is irrational. Doubtless this is not the authors' intent. A more tenable line of argument would be that an increase in rationality pro-

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[27] The problem raised by such an index is that it is difficult, probably impossible, to ascertain which permutation of the components is reflected in a particular instance of a given value of the index, yet it is by no means clear that a given value of the index has the same theoretical relationship to the independent variables, regardless of the permutation of components upon which it is based.

[28] Maccoby, Matthews, and Morton, *op. cit.*, p. 38.

TABLE VIII. DEFECTION AS A FUNCTION OF EDUCATIONAL LEVEL[a]

	0	1	2	3	4	5	6	7	8	—	Total
Defection	0	29	42	49	5	58	31	28	19	1	262
Loyalty	2	74	90	109	14	146	63	86	66	3	653
Total	2	103	132	158	19	204	94	114	85	4	915
% Defecting $p > .05$ $r_b = 1.0\%$	0.0	28.2	31.8	31.0	26.3	28.4	33.0	24.6	22.4	25.0	28.6

[a] The code for educational level is as follows:
0 = none
1 = some grade school
2 = completed grade school
3 = some high school
4 = some high school & other non-college
5 = completed high school
6 = completed high school & other non-college
7 = some college
8 = completed college
– = not ascertained

Note: Those for whom educational level was not ascertained constitute a violation of the rule that only those about whom full information was available—albeit an inadvertent violation. Where these four enter into calculations, and that is only when education is dichotomized, they are treated as though well educated.

duces an increase in defection from traditional identifications in some direct proportion to the extent to which those identifications are irrational. Such a line of argument is readily supported by the present empirical findings relating defection to sociological deviance in party identification with respondent's education as a catalyst in the relationship.

VII. TESTING THE EXPLANATION

It now remains to inquire into the tests of the "rationality" model. The reader may recall that in addition to the general utility maximization model, this approach assumes that group-based norms regarding party identification are rational and that education facilitates utility maximization by reducing information and calculation costs. If education has this catalytic effect, sensitizing the respondent to the irrationality in paternal deviance from group norms of party identification, it ought to be the case that the proportion of defection which occurs during the high school and college years is greater among respondents whose fathers have sociologically deviant party identifications than where those identifications are normal. If sociological norms are rational, and if education sensitizes one to this rationality, then it ought to be possible to predict party identification from sociological characteristics with greater accuracy for well educated than for poorly educated people. Finally, if the better educated, to a greater extent than the poorly educated, do use sociological referents in maintenance of their party identifications, this should be reflected in differences between these two groups in the strength of association between sociological characteristics and partisan attitudes. It was against these deductions that the "rationality" model was tested by way of their goodness of fit with available evidence.

In order to test the first deduction, i.e., that the high school and college years account for a greater proportion of the defection which occurs where paternal party identification was deviant than of the defection which occurs when such identification was normal, it was necessary to establish the respondent's age at the time of defection. This had to be done in an indirect way and a certain problem of inference developed. The body of data being analyzed permitted one to establish the respondent's recollection of his father's party affiliation, the respondent's own present party identification, whether or not the respondent had ever identified himself with a party other than the one with which he was currently identified, if so, when he had assumed his current identification, and his age. From these data one could establish whether the respondent had a party identification different from that of his father. If so, the respondent was classified as a defector ,and became of interest for present purposes. His age at defection was established on the bases of the year in which he had assumed his current identification and of his age in 1956. However, more than half of those who did not identify with the same party as that with which they identified their fathers, claimed *never* to have identified themselves as supporters of a party other than their current one.

The work of Greenstein and others suggests a substantial recall error here, perhaps because of respondents' confining themselves to their adult years in tracing their histories of party identification.[29] The data are presented in Table IX. The grouping of the data in the Table into five-year blocs was done for convenience, but the reader should bear in mind that those classified as defecting between birth and the end of the fourth year are actually those who claim never to have identified with the father's party.

An examination of the table reveals that the high school and college years (ages 15–24) account for 15/118ths or 12.7% of the defection which occurs where paternal party identification was sociologically deviant, and 16/144ths or 11.1% of the defection where such identification was normal. Although the difference in proportions is in the expected direction, the difference is so small as to have a very high probability of occurrence by chance. At this point, those respondents who claim *never* to have identified with the paternal party become a source of concern. If one assumes (and it is a rather arbi-

TABLE IX. AGE AT TIME OF DEFECTION AS A FUNCTION OF DEVIANCE IN PATERNAL PARTY IDENTIFICATION

		Frequency Distributions														
		Age at Defection														
		0–4	5–9	10–14	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65+	Total
Paternal Party Identification	Deviant	72	0	8	8	7	8	3	4	0	6	0	2	0	0	118
	Normal	68	2	13	9	8	18	4	11	6	1	3	0	0	1	144

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[29] See n. 10, above.

trary assumption) that these respondents mean *never during their adult years,* then one may possibly infer that their defections occurred between the ages of 15 and 24. If these people are included in the previous analysis, then the high school and college years account for 87/118ths or 73.7% of the defection which occurs where paternal party identification was sociologically deviant as against 85/144ths or 59.0% of the defection where such identification was normal. This difference in proportions is not only significant at the .05 level, but has a probability of occurrence by chance of only 0.0132. However, despite this, the rather arbitrary assumption made with regard to those responding "never," requires one to conclude that at best the deduction being tested was not refuted by the data. To say that it was supported by the data would be unwarranted.

The next deduction to be tested was somewhat more straightforward, and the results were also rather more straightforward. If the sociological norms of party identification are rational, and if education enables one more readily to perceive this rationality, then one ought to be able to predict party identification from sociological characteristics with greater accuracy for the better educated half of the sample than for the less well educated half. With the sample thus divided at the median level of education into two groups, a linear multiple regression was run in each group in which respondent's party identification was regressed upon his religion, self-identification as working or middle class, community size (farm, town, metropolis), region (South, non-South), and race. In the better educated half of the sample, these characteristics accounted for 26% of the variance in party identification, whereas among the less well educated half of the sample only 7% of the variance was accounted for by these characteristics. Thus one can safely say that the data here support the deduction, in that sociological characteristics account for very nearly four times more of the variance in party identification among the well educated than in the case among the poorly educated. This was the strongest single finding supportive of the explanatory model, but the third deducation remained to be tested in order to shed light upon the cognitive processes involved. While sociological characteristics appeared to be rational guides to action, was this reflected in the attitudes of the respondents? Inquiry was made into this via the relationship between partisan attitudes and sociological characteristics.

Ascertaining the extent to which partisan attitudes reflect sociological characteristics was something of a problem. Had each of these been a single variable, the problem would have been relatively simple, lending itself to analysis by way of correlations. In the actual event, partisan attitudes consisted of four variables, and sociological characteristics were five separate variables.[30] The approach taken was to ask to what extent predictions of party identification based upon sociological characteristics lead to the same set of expectations as those generated by basing such predictions upon partisan attitudes. This approach was implemented by using the two sets of predicted values generated by regressions of party identification upon sociological characteristics and partisan attitudes respectively. Each of these sets of predicted values was then dichotomized at 0.500. Within each set, those with predicted values greater than 0.500 were classified as "expected Republicans", and those with predicted values less than 0.500 were classified as "expected Democrats." These two sets of expectations regarding party identification—one set based upon sociological characteristics and the other set based upon partisan attitudes—were then cross-tabulated and the Goodman and Kruskal tau measure of association used to establish how much information expectations based upon sociological characteristics contained about expectations based upon partisan attitudes.[31] This entire procedure was implemented separately in each of the two educational groups. The results are presented in Table X.

As can be seen in Table X, there is very nearly five times as much information about the dependent variable in the independent variable among the well-educated as among the poorly-educated. (Goodman and Kruskal's tau is 11.0% in the former instance and no more than 2.4% in the latter instance.) Thus, the findings support the idea that sociological considerations are a more salient referent for the formation of partisan attitudes among the well-educated than among the poorly-educated, and hence, they support the explanatory model being offered. However, given that even among the well-educated, the tau is only 11%, it seems clear that sociological considerations, as herein measured, are only a part of the total set of rational considerations.

VIII. SUMMARY, CAVEATS, AND CONCLUSIONS

The inquiry began with the discovery of two rather unanticipated relationships in the data.

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[30] For operational procedures see: Arthur S. Goldberg, "Discerning a Causal Pattern Among Data on Voting Behavior," this REVIEW, 60 (December, 1966), 922.

[31] Cf. Blalock, *loc. cit.*

TABLE X. PREDICTION OF PARTY IDENTIFICATION FROM SOCIOLOGICAL CHARACTERISTICS AND FROM PARTISAN ATTITUDES

Given: High Education

		Prediction Based Upon Sociological Characteristics		
		Republican	Democratic	Total
Prediction Based Upon Partisan Attitudes	Rep.	122	63	185
	Dem.	63	143	206
	Total	185	206	391

$r_b = 11.0\%$

Given: Low Education

		Prediction Based Upon Sociological Characteristics		
		Republican	Democratic	Total
Prediction Based Upon Partisan Attitudes	Rep.	53	48	101
	Dem.	60	119	179
	Total	113	167	280

$r_b = 2.4\%$

First, the evidence suggested that sociological deviance in party identification on the part of the respondent's father was an important source of intergenerational defection in party identification. Moreover, and of central importance here, it was discovered that this relationship was stronger among the better educated than among the less well-educated respondents. An explanation was generated for the second finding. This explanation consisted in the utility maximization model plus three assumptions: 1) that group-based norms regarding party identification produce effective rationality with regard to the goals of the group; 2) that the individual in the group shares the goals of the group; 3) that education increases rationality in the special sense of reducing the costs of information and of calculation. This approach explained the findings in that, under the assumption of the approach, one would expect the well educated to perceive more accurately than the poorly educated the expected utility associated with the paternal party identification. One would expect, under these assumptions, that sociologically deviant party identification would have a lower expected utility than sociologically normal party identification. The findings, then, become anticipated results under the assumptions of the explanatory model.

The explanatory model was then tested by testing deductions made from it against available data. Three deductions were made and tested: first, that the proportion of defection occurring during the high school and college years would be greater where paternal party identification had been sociologically deviant than where it had been sociologically normal; second, that one would be able to predict party identification on the basis of sociological characteristics with greater accuracy for the better educated than for the less well educated halves of the population; finally, that attitudes toward each of the two parties would reflect sociological characteristics to a greater extent among the well educated than among the poorly educated halves of the population. As tested, all of these deductions were sustained by the data. However, the strengths of association and differences in proportions involved in the testing, while always in the predicted direction, and while in all cases, save one, statistically significant at the 0.05 level, are nonetheless small enough to preclude seriously considering the inquiry closed.

As the matter now stands, the present analysis seems to suggest that there is a rational component to sociological norms of party identification, but that there are most probably other rational considerations as well. In the absence of such additional considerations, the validity of the rationality model can only be defended by attributing *all* of the unexplained variance in each of the tests to measure error. Both an extension of the model to include other rational considerations, and a reduction in measurement error seem apt prescriptions for further research in this area.

At least three modifications should be made in the model. First, and perhaps most obviously, the model should take account of the fact that there are many routes—other than education—to the reduction of information costs. Occupational roles, leisure time activity, location in formal and informal communication networks, all produce an effect upon information costs beyond that accomplished by education. Second, the model should be made dynamic. That is, it should be able to take into account the fact that policy outputs through time entail shifts in the relative advantages or deprivations to various components of the electorate, and that rational group norms would change accordingly. Finally, as a model of human dynamics, provision ought to be made for the time lag involved in learning that an old norm is no longer functional. One might go further and recognize that there are very likely two time lags, during the first of which opinion leaders learn that the old norm is no longer desirable and during the second of which the new prescription offered by these leaders are being considered and tested by their followers.[32] Indeed, one might even wish to distinguish between opinion leaders and followers by the factors which receive heavy utility weightings in their respective calculations. For opinion leaders, policy considerations might be taken to outweigh considerations of interpersonal affect. For followers, on the other hand, affective consideration might receive priority. Certainly, other modification may prove warranted, but these ought to provide at least a beginning.

Serious efforts are also required in the matter of reducing measurement error. In the earlier discussion of measuring "deviance" in paternal party identification it was noted that the present study treats a statistical norm as a felt norm and that these two are not necessarily equivalent. There are at least two solutions to the reduction of this aspect of measurement error. For purposes of testing the model, recourse ought to be had once again to community, or perhaps even subcommunity studies. Here one could not only more accurately measure norms, but could trace information flows much more readily than with national samples. On the other hand, for inferring national parameters, the simulation approach of Pool, Abelson, and Popkin seems more appropriate.[33] However, the problems of the simulation approach are formidable, most particularly if one recognizes the dynamics operative in the system being approximated by the simulation. It may be recalled that the authors of the Simulmatics Project used "common sense, social science theory, and similar guides" and thus expressed their "best judgment on how different types of voters would respond to a campaign focused on the religious issue."[34] These judgments were assessed aginst the extent to which they led to correct results when applied in the equations of the model.[35] However, as the authors themselves point out, the inference of national parameters in a complex model entails many difficulties, most especially if the dependent variable is dichotomous. No single trial can genuinely test the estimated parameters, even if the model is assumed correct, in as much as many permutations of the parameters would produce the same results.[36] Many deductions, entailing other than dichotomous outcomes, must be tested in many trials, before one can reasonably begin to believe the estimates of the parameters. However, if these parameters themselves are in flux, the problem becomes substantially more complicated, to say the least.

An even more formidable source of measurement error inheres in the identification of salient group norms. As social scientists, investigators are almost certain to be middle class people. As political scientists, they are apt to be atypically preoccupied with policy outputs. How likely, then, are such investigators to design a questionnaire which captures the salient events used by a slum dweller in his rational calculus. Even with regard to middle class norms, there are substantial areas of ignorance. What statistically based inference can one make from the 1956 election data about middle class values which will explain the Eisenhower victory and the Republican Congressional defeat in that election?[37] The problem of mapping and weighting preferences in the electorate is still rather far from solved. Thus, one can expect tests of the model to be contaminated by measurement error for some time to come.

Despite all these qualms, the present study strongly suggests that there is a rational component to party identifications rooted in group norms. At least in retrospect, the notion is an obvious one. If one considers the origins of group norms, one expects that at their inception they were rational means to group ends. It may not then be surprising to find that they continue to be rational guides to action, even when learned on primarily affective bases. It might not be untoward, then, for research to be addressed to a world of rational voters seeking to reduce information costs by following group norms, which have in the past seemed effective, with due allowance made in the analytical model for the learning and adaptative processes incumbent upon the voter in attempting to cope with a changing political environment, as well as with his sundry immediate and perhaps more pressing responsibilities.

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[32] Cf. William N. McPhee, *Formal Theories of Mass Behavior* (London: Collier-Macmillan Ltd., The Free Press of Glencoe, 1963), Chapter II. See also William N. McPhee and William A. Glaser, *Public Opinion and Congressional Elections* (New York: The Free Press of Glencoe, 1962), esp. pp. 139–151.

[33] Ithiel de Sola Pool, Robert P. Abelson, and Samuel L. Popkin, *Candidates, Issues, and Strategies* (Cambridge, Massachusetts: The M.I.T. Press, 1964).

[34] *Ibid.*, pp. 54–56.

[35] *Ibid.*, pp. 56–57.

[36] *Ibid.*, pp. 102–106.

[37] Despite Key's laudable efforts in this direction in *The Responsible Electorate* (see for example, pp. 79–89 of that work), I suggest that the question remains open.

APPENDIX:

An Evaluation of Some of the Literature on Social Mobility and Intergenerational Defection

Let us examine the literature which Riecken cites regarding the impact of social mobility on partisanship. The first piece of evidence cited in this connection is *Voting*, which presents a chart entitled "The Child's Correspondence with the Father's Vote is Affected by His Relative Status in the World."[38] Table A1, below, is abstracted from that chart (See Fig. A1) and presents information on the impact of upward mobility on the offspring of Democratic fathers. The tau produced by this array is 0.5% and $p > .05$. Thus, as presented in *Voting*, the impact of social mobility on voting behavior is at best very small, as illustrated in Table A1.

Maccoby, Matthew, and Morton in their article "Youth and Political Change" are also interested in the relationship between social mobility and partisan preference.[39] However, their central concern is with the phenomenon of adaptation to group norms. Their data do seem to support the contention that upward mobile persons tend to manifest the norms of the social strata into which they are moving to a greater extent than downward mobile persons.[40] Unfortunately, their data are presented in such a way that it is impossible to know the relevant rates of intergenerational defection, or, for that matter, the extent to which the percentage differentials represent any kind of defection at all. The relevant table is reproduced as Table A2, below.

TABLE A1. INTERGENERATIONAL DEFECTION FROM DEMOCRATIC PATERNAL VOTING PREFERENCE AS A FUNCTION OF OCCUPATIONAL MOBILITY[a]

	Child's Occupation		
	Higher than Father	Same as or Lower than Father	Total
Defection	28	41	69
Loyalty	33	63	96
Total	61	104	165
% Defecting $p > .05$ $\tau_b = 0.5\%$	45.9%	39.4%	41.8%

[a] Abstracted from Chart xxxix in Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954), p. 91.

The most directly relevant and, in many ways, the most substantial set of findings supporting social mobility as a major factor in intergenerational defection is that produced by McClosky and Dahlgren.[41] The relevant table from their article is presented as Table A3, below. Among those whose family preference is Republican, the relationship between respondent's social mobility and his party preference yields a tau of 13.5% $(p < .01)$; among those whose family preference is Democratic, the same relationship yields a tau of 8.6% $(p < .05)$. In comparison with the strengths of association developed in the present study, these are sufficiently large to warrant a reconsideration of the assertion that social mobility is not a significant factor in accounting for intergenerational defection. Yet, a careful examination of Table A3 reveals that McClosky and Dahlgren are utilizing the term social mobility in a rather unusual way. They do not use the trichotomy "upward-none-downward," but rather a trichotomy "favorable-neutral-antagonistic" to describe their categories of social mobility. An examination of the marginals strongly suggests that these are *not* equivalents. For example, if where the parental preference was Democratic, "antagonistic" is taken to mean upward mobility, then only seventeen percent of the offspring of Democratic parents have been upwardly mobile. Moreover, if "antagonistic" is taken to mean upward mobility in this case, then "favorable" ought to mean downward mobility; on this basis 45% of the offspring of Democratic parents have been downwardly mobile. Since recent studies suggest that upward mobility in the United States runs at about 40% and downward mobility at about 25%,[42] and since in the present study, about 40% of the offspring of Democratic fathers are upwardly mobile, as compared with about 30% of such offspring who are downwardly mobile, it seems most unlikely that "favorable-antagonistic" is in any sense a direct analogue to "upward-downward" or "downward-upward." Rather, it would seem that McClosky and Dahlgren have included in the "favorable" and/or "neutral" categories a

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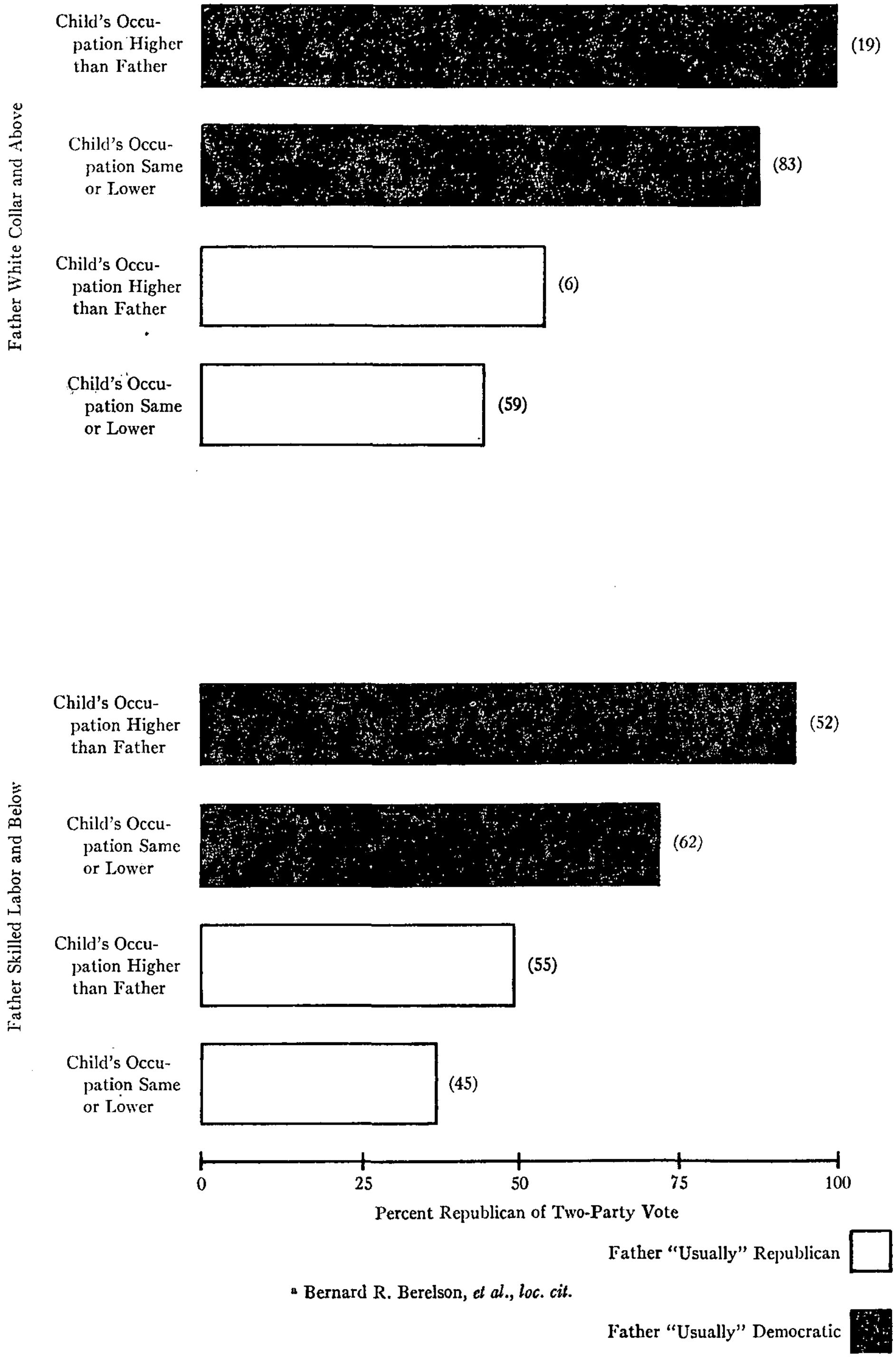
[38] Berelson, Lazarsfeld, and McPhee, *op. cit.*, p. 91.

[39] Maccoby, Matthews, and Morton, *op. cit.*

[40] *Ibid.*, pp. 33–36.

[41] McClosky and Dahlgren, *op. cit.*

[42] Elton F. Jackson and Harry J. Crockett, Jr., "Occupational Mobility in the United States: A Point Estimate and Trend Comparison," *American Sociological Review*, 29 (1964), 5–15.



The Child's Correspondence With the Father's Vote Is Affected by His Relative Status in the World[a]

[a] Bernard R. Berelson, *et al.*, *loc. cit.*

FIG. A1.

TABLE A2. SOCIAL MOBILITY RELATED TO CHOICE OF PARTY AND CANDIDATE*[a]

Party preference	Upward Mobile	Non-mobile, in Class Where Upward Mobiles Originated**	Non-mobile, in Class to Which Upward Mobiles Moved**
Republican definitely	18%	8%	21%
Republican leanings	5	—	—
Independent	9	18	28
Democratic leanings	12	1	8
Democratic definitely	56	73	43
	100%	100%	100%
Number of cases	82	73	73
Candidate choice			
Eisenhower	48%	28%	37%
Stevenson	52	72	63
	100%	100%	100%
Number of cases	82	72	72

TABLE A2 (continued)

Party preference	Downward Mobile	Non-Mobile in Class Where Downward Mobiles Originated**	Non-Mobile, in Class to Which Downward Mobiles Moved**
Republican definitely	28%	21%	11%
Republican leanings	3	—	—
Independent	8	28	17
Democratic leanings	4	8	3
Democratic definitely	57	43	69
	100%	100%	100%
Number of cases	75	73	73
Candidate choice			
Eisenhower	46%	38%	27%
Stevenson	54	62	73
	100%	100%	100%
Number of cases	74	72	72

* Students are excluded from this table, since their mobility is still undetermined.

** For purposes of comparison, the non-mobile cases have been weighted to cancel out differences between them and the mobiles in socio-economic status. That is, for the first comparison, the non-mobiles have been weighted so as to have the same SES distribution as the mobile people at their point of origin. For the second comparison, the non-mobiles have been weighted so as to have the same distribution as the mobiles have in their destination class.

[a] Eleanor E. Maccoby, Richard E. Mathews, and Anton A. Morton, "Youth and Political Change," *Public Opinion Quarterly*, 18 (1954–55), 35.

TABLE A3. RELATION BETWEEN VOTER STABILITY, FAMILY PREFERENCES, AND SOCIAL DISTANCE[a]

Family Preference is Republican				Family Preference is Democratic			
	Respondent's Social Mobility is:				Respondent's Social Mobility is:		
Respondent's Preference is	Favorable (Rep)	Neutral	Antagonistic (Dem)	Respondent's Preference is	Favorable (Dem)	Neutral	Antagonistic (Rep)
		(%'s Down)				(%'s Down)	
Republican	84.6	60.8	42.1	Democratic	84.3	62.9	50.0
Democratic	15.4	39.1	57.9	Republican	15.5	37.0	50.0
Sample Size	39	23	19	Sample size	32	27	12
Mean Stability Score*	4.90	3.91	3.32		5.25	4.04	3.50

* The Mean Stability score is computed on a 6 point scale in which 6.0 represents highest party stability in the same direction as the family preference, and 1.0 represents highest stability in the direction of the party opposed by the family.

[a] Herbert McClosky and Harold E. Dahlgren, "Primary Group Influence on Party Loyalty," this REVIEW, 53 (1959), 769.

good deal of upward mobility for offspring of Democrats, and have done the same regarding downward mobility for offspring of Republicans. Thus, for them the upward mobility from unskilled to semi-skilled labor would not be regarded as unfavorable to the persistance of Democratic party preference. This is not necessarily ill advised, in fact it seems quite sound for their purposes—the measurement of support provided by current milieu for parental party preference. However, in such an approach mobility itself is not the explanatory factor. It is neither the fact of mobility itself nor the size of the increment which is important, but rather the thresholds which are crossed. Thus, a high rate of upward social mobility would not necessarily produce a steady flow to the Republican party. Such a flow could be expected only if the mobility occurred across certain critical points, e.g., skilled labor to administrative occupational categories. The McClosky and Dahlgren findings are sound, but their invocation in support of the contention that "upward mobility signals. . . the likelihood that political preference will shift from Democratic to Republican" is a bit misleading, for it is true only with regard to such mobility across certain thresholds. Thus, the literature which Riecken cites does not preclude the acceptance of the findings on social mobility and defection presented in the present study.

CONCEPT FORMATION IN NORMATIVE AND EMPIRICAL STUDIES: TOWARD RECONCILIATION IN POLITICAL THEORY

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Despite the fact that the study of politics has become increasingly empirical, quantitative and "behavioral" in recent years, and despite the apparently increasing tendency to feel that whatever meaningful debate ever existed between the behavioralists and the anti-behavioralists has ended, should end, or at least has become irrelevant since a more sophisticated and empirically productive behavioralism now predominates in virtually all fields of the discipline,[1] the methodological debate continues, diminished perhaps in quantity but not in intensity.[2]

This essay is based on the assumption that the antagonists concerned with the methodological issues raised by the "new science of politics" have but rarely focused precisely on the arguments raised by their opponents. A second motivating assumption is that nothing constructive, conciliatory or conducive to the integration of the discipline can be done "until the issues have been squarely confronted on the basic and general plane of philosophy. . . ."[3] A thorough analysis of all of the meaningful issues involved can only be a task of long-range proportions. But in the hope of bringing about some degree of communication, if not reconciliation, it is my intention in this essay to bring one of these issues into sharper focus, to show that almost despite themselves, some of the critics and proponents of the "new science of politics" have addressed themselves to the problem of concept formation, and that despite their proclaimed differences are talking at cross-purposes about a similar problem. Indeed, it will be seen that the conflict between the "traditionalists" and the "behavioralists" is utterly dependent—in the area of concept formation—upon an outmoded positivistic interpretation of behavioral science and a misguided reaction on the part of some political theorists to that obsolete conception.

Concept formation as a general problem in philosophy, however, is extremely complex inasmuch as it covers questions of definition, classification, comparison, measurement, and empirical interpretation. To achieve our immediate and ironic purpose, however, it is desirable to restrict the analysis to the questions of definition and especially of the criteria of classification to be used in developing the basic concepts of political science, concepts of the attributes of political phenomena. Not only is it necessary to resolve this question prior to engaging in the questions of comparison and measurement, but it is primarily on this most basic level that it is possible to see most clearly the nature of the misunderstandings held by the antagonists on both sides of the controversy about a behavioral science of politics. Once we understand how we develop criteria of identity and classification, problems such as comparison become relatively simple on the conceptual level and permit us to concentrate our energies upon the application of our concepts and techniques to the data we wish to compare or measure.

I. THE "NEW SCIENCE OF POLITICS": OVERT BEHAVIOR AS A PRINCIPLE OF CONCEPT FORMATION

There are two extreme positions that have been taken regarding the nature of concept formation during the recent history of the social sciences. One is the view characteristic of the earlier forms of positivist and empiricist philosophy according to which "any scientific statement, however abstract, could be transformed, by virtue of the definitions of its constituent terms, into an equivalent statement couched exclusively in observation terms: Science would really deal solely with observables."[4] As John Stuart Mill suggested, "the backward state of the moral sciences can only be remedied by applying to them the methods of physical science, duly extended and generalized."[5] For the purpose of scientific inquiry, at least, the behavioral political scientist in the eyes of his critics treats as non-existent all psychic phenomena, especially the valuing activities of human beings, and his concepts must be technical concepts, as remote from everyday, common sense constructs as possible.[6]

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[1] Robert A. Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," this Review, 55 (December, 1961), 763–772.

[2] See Herbert J. Storing (ed.), *Essays on the Scientific Study of Politics* (New York: Holt, Rinehart and Winston, 1962); John H. Schaar and Sheldon S. Wolin, "Essays on the Scientific Study of Politics: A Critique," this Review, 57 (March, 1963), 125–150; Herbert J. Storing, Leo Strauss, *et al*, "Replies to Schaar and Wolin," pp. 151–160; James C. Charlesworth (ed.), *The Limits of Behavioralism in Political Science* (Philadelphia: American Academy of Political and Social Science, 1962); W. G. Runciman, *Social Science and Political Theory* (Cambridge: University Press, 1963), especially pp. 156–175; James C. Charlesworth (ed.), *A Design for Political Science: Scope, Objectives, and Methods* (Philadelphia: American Academy of Political and Social Science, 1966); Eugene J. Meehan, *Contemporary Political Thought: A Critical Study* (Homewood, Ill.: Dorsey Press, 1967), especially pp. 1–110.

[3] Schaar and Wolin, *op. cit.*, p. 125.

Arthur Bentley, of course, is the most emphatic exponent in the recent history of political thought of the idea that the study of politics should confine its inquiry to "observables" or, as Herbert Hochberg puts it, to "overt behavioral patterns."[7] "My epistemological point of view," Bentley claimed, "is admittedly naive, as naive, I hope as the point of view of the physical sciences; I nowhere lay any stress on the difference between the conscious and the unconscious. . . ."[8] This statement, it should be observed, does not refer to the *techniques* of the physical sciences, or to the *logic* of scientific procedure in general. Bentley is stressing a particular way of looking at phenomena, a way of constructing concepts of the objects of inquiry in the science of politics. In a very general sense, Bentley's claim of epistomological identity with the physical sciences is quite justified. The "viewpoint" of the physical scientist is indeed not that of introspection, and color and sound have no meaning in his explanations except in terms of wave lengths, which are theoretical constructs interpreted in terms of the observed behavior of material entities.

Theoretical constructs interpreted in terms of the externally observed behavior of humans is the similar goal of Bentley's political science. For Bentley, ideas and feelings, like color and sound, may be immediately experienced by subjects but cannot be the "raw material" of a science of politics. What, he asks, are "the practical realities" for which these feeling and idea factors stand?

> . . . we readily see that they stand for certain regularities . . . in activity stated as individual conduct. For instance, if a child is kind to its cat it is apt to be kind to its dog. We indicate the tendency by calling the child kind-hearted.
>
> However, while these feelings and ideas put themselves forth to be definite dependable things, experience proves that they only conform roughly to the actual activity that can be observed . . . from the standpoint of the feelings we can observe nothing more than unreliable, poorly defined tendencies of activity to correspond to them. Kindness to cat or dog is not accompanied by kindness to snake or mosquito.
>
> When we get to the application of these feeling and idea elements to social interpretation, our difficulties become greatly increased . . . The words commonly used for feelings and ideas break down, and for the social researcher ideas and feelings must be made so definite in meaning . . . . that they fit the facts of the case like shadows. . . .[9]

The concepts used by the social scientist must correspond not to the meanings imputed to their own conduct by the subjects themselves but rather must be constructs referring and corresponding to the external activity of the subjects as observed by the social scientist. "If we can get our social life stated in terms of activity, and of nothing else . . . we have at least reached a foundation upon which a coherent system of measurements can be built up."[10] This is the position also of Charles E. Merriam, G. E. G. Catlin and other early proponents of behavioral political studies.[11] Essentially the same position has been expressed in more recent polemic. David Apter, for example, argues for the development of "research theory" that is based upon concepts not derived from common sense. "Much of what seems to be a maze of jargon in social science," he points out, "is a direct result of the effort to cut through so-called common sense terms which, upon intensive probing, prove themselves to be of limited usefulness."[12] Apter feels that the epistomology that is implicit in most political research has been that of "historicism," or the attempt to discover the ideas of an historical period and the "structures" in which they are expressed.[13] The historical approach, unfortunately, has placed undue emphasis on the uniqueness of an age and therefore also on the idea that the age can best be understood through participation in "the social processes on one's environment." Contrary to this approach, Apter contends that "the only way in which questions can be phrased in terms of their maximum significance is to articulate the body of research theory to which the problem is relevant."[14] This, in turn, can best be done by developing abstract models of interaction.[15] The variables in these models, of course, will be only those which can be scientifically studied and which are significant in terms of the scientific problems which interest the scholar or the profession, rather than in terms of the political problems important in political life.

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[4] C. G. Hempel, *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago Press, 1952), p. 24. The original statement of this is to be found in J. B. Watson, *Behaviorism,* Revised Edition (Chicago: University of Chicago Press, 1930). Other contemporary philosophers of science describe early, or radical, behaviorism in this manner also. See Felix Kaufmann, *Methodology of the Social Sciences* (London: Oxford University Press, 1944), pp. 148, 151; Ernest Nagel, *The Structure of Science* (London: Routledge and Kegan Paul, 1961), pp. 476–477.

[5] Ernest Nagel, *John Stuart Mill's Philosophy of Scientific Method* (New York: Hafner Publishing Co., 1950), p. 307.

[6] This view of what is implied by the behavioral position is appropriate in regard to J. B. Watson's work, but not with respect to the work of contemporary behavioral scientists. And even exponents of radical behaviorism, while rejecting introspection as a research method, did not deny the existence of conscious mental states. See Ernest Nagel, *The Structure of Science, op. cit.,* pp. 477, 480.

This obsolete and incorrect view of what is implied by the behavioral position is dominant among critics of the "new science of politics." See Leo Strauss, "An Epilogue," in Herbert J. Storing, *op. cit.,* pp. 310–320; Leo Weinstein on Bentley, pp. 154, 174, 188, 189, 220; Herbert J. Storing on Simon, pp. 112, 123–132. See also Henry S. Kariel, "Political Science in the United States: Reflections on One of Its Trends," *Political Studies,* 4 (June, 1956), 114–115.

[7] Herbert Hochberg, "Physicalism, Behaviorism, and Phenomena," *Philosophy of Science,* 26 (April, 1959), 93.

[8] Arthur F. Bentley, *The Process of Government* (Evanston: Principia Press, 1935), p. 177. n. 1.

[9] *Ibid,* pp. 167–168.

II. THE "OLD POLITICAL SCIENCE": VERSTEHEN AS AN INSTRUMENT OF EXPLANATION

At the opposite extreme from the radical behaviorists, who maintain the thesis that concepts in the science of politics are to be developed by the observer on the basis of his perception of the overt actions of his subjects, are those who have argued in favor of what is usually called the "*verstehen* operation."[16] Until recently there was little question but that the term *verstehen* referred to an explanatory technique, the "method" of empathy, or to the intuitive imputation of motives to human agents other than oneself on the basis of one's understanding of one's own motivation. It is the attempt to "understand" social behavior by imputing "springs of action" to the actors involved in it by means of an imaginative identification of the social scientist with the participants in the social situation under analysis. The "external" observation of social behavior, of "patterns of action," from this point of view is at best extremely limited in applicability, and at worst utterly fruitless. The political and social sciences, it is argued, should not, or cannot, be molded along the methodological lines of the physical sciences.

As Theodore Abel describes the "*verstehen* operation"[17] it involves the assumption that the social scientist has within the universe of his experience feelings or emotions which he has subjectively correlated with certain behavior he himself manifested in the past. The social observer also associates, often but not always quite logically, certain groups of value concepts with others—for example, a value concept asserting the value of life might be associated with other attitudes such as, but not necessarily, pacifism, toleration, and opportunities for individual fulfillment. Then these feelings which are meaningfully associated with each other in the individual are used by him as an investigator to "explain" the behavior of others.

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[10] *Ibid,* pp. 169, 184–185.

[11] Charles E. Merriam, *New Aspects of Politics* (Chicago: University of Chicago Press, 1925), p. 86; "Progress in Political Research," this REVIEW, 20 (Feb., 1926), p. 7. As G. E. G. Catlin put it, "Politics must view social phenomena externally, considering without moral *parti pris* the appraisals of value and conventional estimates which it observes that men, in given places or times, in fact entertain. . . . The appreciation of the meaning of the phenomena it must leave to art and religion, which alone are competent to give us 'empathically' a sympathetic understanding of the personal experience": *The Science and Method of Politics* (New York: Alfred A. Knopf, 1927), p. 106. For others see the discussion in Bernard Crick, *The American Science of Politics* (London: Routledge and Kegan, Paul, 1959), p. 129.

[12] David Apter, "Theory and the Study of Politics," this REVIEW, 51 (Sept., 1957), p. 755.

[13] *Ibid,* p. 755.

[14] *Ibid,* p. 755.

[15] *Ibid,* p. 757.

[16] The most succinct analysis of this is given by T. Abel, "The Operation Called *Verstehen,*" *American Journal of Sociology,* 54 (Nov. 1948), 211–218. See also Maurice Natanson, "A Study in Philosophy and the Social Sciences," *Social Research,* 25 (Summer, 1958), 158–172. In political science the best-known proponent of *verstehen* as a necessary instrument of explanation in social science is Leo Strauss, *Natural Right and History* (Chicago: University of Chicago Press, 1953), pp. 50–59.

[17] Abel, *op. cit.,* pp. 211–218.

In political science, Leo Strauss, expanding in large part upon the methodological position of Max Weber, agrees with him that human conduct cannot be understood ". . . without being aware of the standard of judgment that is inherent in the situation and accepted as a matter of course by the actors themselves. . . ."[18] He draws a strong distinction between what he calls "the new science of politics" and the old, or Aristotelian, political science on the basis of several criteria, including the notion that ". . . Aristotelian political science views political things in the perspective of the citizen [prior to judging or evaluating those various perspectives on the basis of ". . . a more comprehensive and a clearer grasp of man's natural ends and their natural order. . . ."] whereas the new political science ". . . looks at political things from without, in the perspective of the neutral observer . . . ."[19] Weber himself, of course, believed that *verstehende* was a method that enabled the social scientist to avoid injecting value judgments into his studies by accepting the self-interpretation of his subjects in the construction of ideal types to be utilized in explanation. For Strauss, however, this aspect of Weber's approach creates a corrupt methodology for the social scientist, for he then falls "victim to every deception and every self-deception of the people one is studying. . . ."[20] This is also the position taken by Eric Voegelin, David G. Smith, Charner Perry, Harry V. Jaffa, Henry S. Kariel, to a lesser extent Bernard Crick and, with the emphasis upon methodological as opposed to historical relativity, by Robert M. Barry.[21] From this viewpoint, then, objectivity is impossible, and Weber's own work is an illustration of the fact that it cannot be achieved. In trying to avoid value judgments, Weber merely developed his analyses on the basis of the conceptual framework of his own society, his own historical milieu. Such historical relativity vitiated Weber's attempt to achieve a value-free social science. *Verstehen,* coupled with the desire to avoid the corruption of historical relativity, forces the social scientist, *qua* scientist, to make value judgments of a moral kind.

This position is also part of the broader methodological stance taken by Robert M. MacIver, who has argued that the verification of hypotheses in the scientific study of politics takes place on two levels. First of all, statistical analyses and related techniques are to be employed to determine whether and to what extent the phenomena under investigation are associated with other phenomena. All hypotheses must pass those tests before they can be considered for further study. Those hypotheses which meet this requirement must then be analyzed on the second level, where the political scientist tries to discover whether the association or correlation is *meaningful.* At this level the evidence is

> . . . the depositions, avowals, confessions, justifications, and other testimonies offered by agents, participants, or witnesses, professedly or ostensibly giving their own answers . . . to the question of causation . . . We must here essay the task of projecting ourselves by sympathetic reconstruction into the situation as it is assessed by others. . . .[22]

An even more radical criticism of the idea of a strictly behavioral science of politics that is based on the notion that common-sense criteria determine the concepts of the study of politics has been put forward by Charner Perry. It is more radical because it assumes that the application of the scientific method to a given subject matter

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[18] Leo Strauss, *op. cit.,* p. 54.

[19] Herbert J. Storing, *op. cit.,* p. 310. See also Leo Strauss, "The Social Science of Max Weber," *Measure,* 2 (1951), 211–214; *The Political Philosophy of Hobbes* (Oxford: The Clarendon Press, 1936), pp. 129–170.

[20] Leo Strauss, *Natural Right and History,* p. 55.

[21] Eric Voegelin, *The New Science of Politics* (Chicago: University of Chicago Press, 1952), pp. 20-27; David G. Smith, "Political Science and Political Theory," this REVIEW, 51 (Sept., 1957), p. 739; Charner Perry, "The Semantics of Political Science," this REVIEW, 44 (June, 1950), p. 398; Harry V. Jaffa, "The Case Against Political Theory," unpublished paper presented to the annual meeting of the American Political Science Association, September 12, 1959, p. 2; Bernard Crick, *op. cit.,* pp. 120–122, 126, 128–129; Robert M. Barry, "Review of Alfred Schutz, *Collected Papers I: The Problem of Social Reality,*" in *International Philosophical Quarterly,* 3 (September, 1963), p. 649.

[22] R. M. MacIver, *Social Causation* (Boston: Ginn and Co., 1942) pp. 390–391. The dependence of the political theorist on "self-awareness and introspection" is asserted also by Norman Jacobson, "The Unity of Political Theory: Science, Morals, and Politics," in Roland Young (ed.), *Approaches to the Study of Politics* (Evanston: Northwestern University Press, 1958), p. 120, and David G. Smith, *op. cit.,* p. 742.

> . . . involves a transformation of the subject matter, a substitution of one point of view and set of terms for another . . . briefly, for the common sense universe of discourse . . . there must be substituted a system of terms, concepts or categories which abstract from human purposes, needs and ideals, in Veblen's terms, concepts which disclose a 'colorless impersonal sequence of cause and effect.'[23]

Because in social behavior language is not merely an instrument, external to the behavior, "but is rather the essential stuff or structure of the behavior, . . ."[24] common sense is teleological and anthropomorphic whereas science, though it originates in common sense, ". . . in its explicit theory . . . uses terms and distinctions which are detached from reference to human desires, needs, or activities."[25] This difference, according to Perry, makes it impossible to apprehend social behavior in the categories necessary in science. While there have been many attempts to describe social action in terms appropriate to natural science, especially in recent years by the behavioralists, Perry considers them all ". . . abortive because the distinctions and preconceptions of common sense have quickly reimposed themselves even when mutilated by a strained terminology."[26] Taking the position that he does, it is necessary to stress the fact that Perry is not in his own eyes arguing for the subjective character of political inquiry. He often speaks of the "knowledge" and "facts" we have of political behavior, and toward the end of his analysis he specifically raises the question, How do the terms of social science have objective reference? He answers, however, in terms similar to those of the other critics of "the new science of politics," for he believes that to describe behavior adequately one must do so in terms of norms, in terms of the purposive activity involved, in terms of the common-sense perspective of the agent.[27]

III. VERSTEHEN AS A PRINCIPLE OF CONCEPT FORMATION

Among philosophers the meaning of the term *verstehen* is still at issue.[28] That interpretation which treats it as referring to *how* one knows or explains the motives of others, that is, to intuition as an instrument of scientific explanation, has been increasingly called in question. *Verstehen,* to philosophers who hold this view, such as Nagel and Hempel, is considered to be a *subjective process* because they hold that understanding the motives of another man's action by means of this procedure depends upon the private, uncontrollable, and unverifiable intuition of the observer. Recent criticism of this view emphasizes an aspect of the concept *verstehen* that is most significant for the problem of concept formation. It also brings the concept closer to its usage as originally established by Max Weber.

Contrary to Nagel and Hempel, one critic asserts that "social scientists, such as Max Weber . . . call *Verstehen* subjective because its goal is to find out what the actor 'means' in his action, *in contrast to the meaning which this action has for the actor's partner or a neutral observer.*"[29] *Verstehen* literally means simply

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[23] Charner Perry, "The Semantics of Political Science," *op. cit.,* p. 398.

[24] *Ibid,* p. 395.

[25] *Ibid,* p. 397.

[26] *Ibid,* p. 399.

[27] *Ibid,* p. 406.

[28] See, for example, Thelma Lavine, "Note to Naturalists on the Human Spirit," *Journal of Philosophy,* 50 (February, 1953), 145–154; Ernest Nagel's criticism of Lavine's position in his article, "On the Method of Verstehen as the Sole Method of Philosophy," *Journal of Philosophy,* 50 (1953), 154–157; and Lavine's reply, "What is the Method of Naturalism?" *Journal of Philosophy,* 50 (1953), 157–161. A year later Carl Cohen joined the debate with an article on "Naturalism and the Method of *Verstehen,*" *Journal of Philosophy,* 51 (April 1, 1954), 220–225.

[29] Alfred Schutz, "Concept and Theory Formation in the Social Sciences," *Journal of Philosophy,* 51 (April, 29, 1954), p. 265. Underlining added. Schutz further observes that the controversy about *verstehen* ". . . suffers from the failure to distinguish clearly between *Verstehen* (1) as the experiential form of common sense knowledge of human affairs, (2) as an epistemological problem, and (3) as a method peculiar to the social sciences." Type (2) represents the way the critics of *verstehen* deal with its meaning, that is, as a problem in *how we know, in general,* viz., by intuition or by observation. Type (3), on the other hand, assumes that the logic implicit in the physical and the social sciences is the same, and treats the term as referring to a problem in method peculiar to the social sciences. That is to say, granted that the logic of all the sciences is the same, nevertheless, social reality, the "object of inquiry" in the social sciences (interaction, intersubjectivity, language) requires a *method of observation* (and therefore a type of concept formation) suitable for those objects, a method that *observes actions as they are meant by the actors* and not that observes actions with meanings imposed by the observer.

See Max Weber, *The Theory of Social and Economic Action* (Glencoe: Free Press, 1957), pp. 9, 18, 22, 90, but esp. p. 88; Talcott Parsons, *The* to understand or to comprehend, and thus both usages are plausible. The interpretation of the term by Schutz, however, stresses the goal, or object, of understanding—*the actor's meaning* —rather than *the process of understanding in the mind of the observer.*[30]

This revised interpretation of *verstehen,* by treating the "subjectively intended meaning" of an action as a problem in concept formation within the social sciences rather than a problem of how we know in general, by intuition or by observation, makes it possible for proponents of the necessity for taking the meaning of action into account to agree with the logical positivists about the nature of the *logic* of the social sciences. Schutz, for example, agrees with Nagel that *all* empirical knowledge depends upon discovery through processes of controlled inference, must be capable of being verified by anyone who will execute the proper experiment, and that any method that would require the scientific observer to select and interpret facts in terms of his private value system would never produce a scientific theory.[31] Nevertheless, he submits that to the best of his knowledge no "... social scientist of stature . . . ever advocated such a concept of subjectivity as that criticized by Professor Nagel. Most certainly this was not the position of Max Weber." And he goes on to assert that Nagel and Hempel

> . . . are prevented from grasping the point of vital concern to social scientists by their basic philosophy of sensationalistic empiricism or logical positivism, which identifies experience with sensory observation and which assumes that the only alternative to controllable and, therefore, objective sensory observation is that of subjective and, therefore, uncontrollable and unverifiable introspection.[32]

This identification of experience with sensory observation in general and of the experience of overt action in particular excludes a great many of the more significant dimensions of social reality from all possible inquiry, as Schutz makes clear by listing several examples:

> a) Even an ideally refined behaviorism can only explain the behavior of the observed, and not of the observing, behaviorist. (This, he says, has been shown by George H. Mead in *Mind, Self and Society.*)
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> b) The same overt behavior (say, a tribal pageant as it can be captured by the movie-camera) may have an entirely different meaning to the performers of different tribes. What interests the scientist is precisely whether it *is* a war dance, a barter [trade], the reception of a friendly ambassador, or something else of this sort.
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> c) 'Negative actions,' that is, intentional refraining from acting, escapes sensory observation. (See Max Weber's *Theory of Social and Economic Organization,* p. 88.) Not to sell certain merchandise at a given price is doubtless as economic an action as to sell it.
>
> d) As W. I. Thomas has shown in *Social Behavior and Personality* (E. H. Volkart, ed., 1951, p. 81), social reality contains elements of belief and conviction which are real because so defined by the participants and which escape sensory observation.
>
> e) Finally, and this is deemed most important by Schutz, the postulate of sensory observation of overt human behavior takes as a model a particular and relatively small sector of the social world, namely, situations in which the acting individual is given to the observer in what is commonly called a face-to-face relationship. But, there are many other dimensions of the social world in which situations of this kind do not prevail—e.g., mailing a letter assumes faith in a vast system of social relationships we do not observe; as does reading an editorial about the French fearing the rearmament of Germany. In all these cases we know what is meant without observing overt behavior.[33]

In order to take into account all of the di-

Structure of Social Action (Glencoe: Free Press, 2nd Edition, 1949), esp. pp. 82 ff, 345–347, 484 ff; Felix Kaufmann, *op. cit.,* pp. 166ff.

[30] Peter Winch makes the similar claim that critics of Weber have misunderstood him. Winch claims that *Sinn* is the subjectively intended sense or meaning which an act has for the actor, and *verstehen* is to understand that, and not to have some special inner sense, or intuition, of causal relations. Weber explicitly asserted the necessity of verifying hypotheses through statistical laws based on observation of what happens. Peter Winch, *The Idea of a Social Science* (New York: Humanities Press, 1958), pp. 117–119, 112–113.

[31] Alfred Schutz, *op. cit.,* pp. 260–261.

[32] *Ibid,* p. 261.

[33] *Ibid,* pp. 262–263. According to Parsons, social interaction always involves ". . . the normative and the conditional. As process, action is, in fact, the process of alteration of the conditional elements in the direction of conformity with norms. Elimination of the normative aspect altogether eliminates the concept of action itself and leads to the radical positivistic position. Elimination of conditions . . . equally eliminates action and results in idealistic emanationism." And, as he points out, normative elements can only be conceived as existing in the mind of the actor, and are thus "inherently subjective." Talcott Parsons, *op. cit.,* p. 732.

mensions of social reality, therefore, Schutz argues that the social scientist must make use of his "common sense knowledge of everyday life." "This is so, because the world (the natural and the social one) is from the outset an intersubjective world and because . . . our knowledge of it is in various ways socialized."[34] Having nothing to do with introspection as a method opposed to scientific verification, *verstehen* is a result of processes of learning or acculturation. Moreover, according to Schutz, it is by no means a private affair of the observer which cannot be controlled by other observers. In his interpretation, *verstehen* is controllable and determinable at least to the same extent as, for example, the private perceptions of facts and the "intent" of the defendant in trials for murder.[35]

> The world of nature, as explored by the natural scientist, does not 'mean' anything to the molecules, atoms, and electrons therein. The observational field of the social scientist, however, namely the social reality, has a specific meaning and relevance structure for the human beings living, acting, and thinking therein. Thus, the constructs of the social sciences are, so to speak, constructs of the second degree, namely constructs of the constructs made by the actors on the social scene, whose behavior the social scientist has to observe and to explain in accordance with the procedural rules of his science.[36]

This analysis makes it plain that there is an essential difference between the "thought objects" or mental constructs formed by the social sciences and those formed by the physical sciences.[37] The adoption of the principle of concept formation described above—namely, that classifications of social action must be in terms of concepts which are meaningful to the agent as well as to the observer—is therefore the only course of action open to the student of social behavior and institutions. But this in no way implies a non-behavioral position regarding the problem of verification. It is not that a different *way of knowing* (namely, "scientific" as against "intuitive") is at the basis of the differences between the physical and the social sciences, but that the *object* of knowledge is different. The social sciences must develop concepts of the intentional dimension of social reality. Thus, as Natanson recognizes, the "subjectivism" of Weber's postulate of interpretation does not mean that private, unverifiable elements are involved in the verification of hypotheses. As he quite correctly puts it, "it is concerned rather with the conceptual framework within which social reality may be comprehended."[38]

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[34] Alfred Schutz, *op. cit.,* p. 263. This "intersubjectivity" appears quite clearly as Schutz points out, in the self-correcting process of scientific inquiry itself. Science is a social enterprise, "but the postulate to describe and explain behavior in terms of controllable sensory observation stops short before the description and explanation of the process by which scientist B controls and verifies the observational findings of scientist A and the conclusions drawn by him. In order to do so, B has to know what A has observed, what the goal of his inquiry is, why he thought the observed fact worthy of being observed, i.e., relevant to the scientific problem at hand, etc. This knowledge is commonly called understanding. The explanation of how such a mutual understanding of human beings might occur is apparently left to the social scientist. But whatever his explanation might be, one thing is sure, namely, that such an intersubjective understanding between scientist B and scientist A occurs neither by scientist B's observation of scientist A's overt behavior, nor by introspection performed by B, nor by identification of B with A." *Ibid,* p. 262.

[35] *Ibid.,* p. 264. The "intersubjective (objective) nature of this learning process is analyzed by Schutz in "Common Sense and Scientific Interpretation of Human Action," *Philosophy and Phenomenological Research,* 14, No. 1 (September, 1953), p. 12. He relates the origin of common constructs to the understanding imparted by communication, which in turn depends on "a community of time and space" between the persons involved. And the latter implies that a certain sector of the outer world is equally within the reach of each person, and that they can interchange positions to acquire the perspective of the other. Significantly, Schutz cites Charles H. Cooley and George Mead on this matter.

The similarity, if not identity, between "social relations" and concepts in general should be obvious at this point. As even Lerner and Lasswell make clear, following Clyde Kluckhohn, in order to understand cultures we must know social relations and *social relations are meanings.* Observed action must be conceptualized in terms of their purposes in the minds of the subjects. Daniel Lerner and Harold D. Lasswell (eds.), *The Policy Sciences* (Stanford: Stanford University Press, 1951), pp. 89–90, 101. And, the meaning of concepts cannot be explained in terms of the actions of any individual person, or persons—they are presupposed, learned, and shared. See Maurice Mandelbaum, "Societal Facts," *British Journal of Sociology,* 6, No. 4 (December, 1955), p. 305; Felix Kaufmann, *op. cit.,* pp. 18–19; Peter Winch, *op. cit.,* pp. 121–123.

[36] Alfred Schutz, "Concept and Theory Formation," *op. cit.,* pp. 266–267. See also Peter Winch, *op. cit.,* pp. 83–85.

[37] This difference appears in the fact that the criteria according to which the student of politics ". . . judges that, in two situations, the same thing has happened, or the same action performed, must be understood in relation *to the rules governing sociological investigation.* But here we run against a difficulty: for whereas in the case of the natural scientist we have to deal with only one set of rules, namely those governing the scientist's investigation itself, here *what the sociologist is studying,* as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist's investigation, which specify what is to count as 'doing the same kind of thing' in relation to that kind of activity." Peter Winch, *op. cit.,* pp. 86–87. If, then, the scholar's criteria of concept formation must come from the rules of action found in political life, his relation

IV. THE PRACTICE OF CONCEPT FORMATION IN THE BEHAVIORAL SCIENCE OF POLITICS

The methodological principle that the concepts of the political scientist must be meaningful to the actor as well as to the observer is a principle that is exemplified in contemporary behavioral research without upsetting the verifiability principle. It would be impossible within the scope of this article to cite as evidence in support of this statement all relevant instances of empirical research accomplished since the turn of the century. Nevertheless, the conclusion that behavioral research conforms to this principle would seem to be justifiable if the two general *types* of inquiry, as illustrated by work which is accepted as representative by leaders of the behavioral movement,[39] were indeed found to conform to this principle of concept formation.

Attitude Studies. One broad area of inquiry which has become prominent among behavioral students of politics has been the study of orientations toward the political process. In this area socio-psychological phenomena such as motives, attitudes, and opinions are analyzed and used to explain such types of behavior as party preferences, voting participation, and the psychological bases of political extremism. The individual is the source of these types of primary data although often the analysis is developed in terms of the significant groups in a given society.

The study of attitudes and motives (relative to political ideology) is conveniently illustrated in the research done in this area by Herbert McClosky.[40] The report which describes these investigations into the relationships between personality attributes and conservative ideology succinctly and lucidly indicates that the procedures used in the formation of his basic concepts are completely in accord with the methodological principle which we have argued is a necessary assumption in social and political inquiry. Recognizing that the term "conservatism" is an important and common one in political life, and one that is used in different contexts to refer to the ideas of Burke, *laissez-faire* liberals, critics of the New Deal, or Republicans of any persuasion, McClosky admits that the definition that he has chosen to employ in his research, and the measure that was constructed from this definition, will not be satisfactory to everyone. However, the major point of interest to us is not the content of his definition but the manner in which he went about constructing it and the subsequent scales based on it. He began this construction by attempting "to extract from the tradition of self-styled conservative thought, and especially from the writings of Edmund Burke, a set of principles representing that tradition as fairly as possible. We have concentrated," he explains, "upon those attitudes and values that continually recur among acknowledged conservative thinkers and that appear to comprise the invariant elements of the conservative outlook."[41]

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to the practitioners of politics ". . . cannot be just that of observer to observed. It must rather be analogous to the participation of the natural scientist with his fellow-workers in the activities of scientific investigation. Putting the point generally, even if it is legitimate to speak of one's understanding of a mode of social activity as consisting in a knowledge of regularities, the nature of this knowledge must be very different from the nature of knowledge of physical regularities." *Ibid.*, pp. 87–88.

[38] Maurice Natanson, *op. cit.*, p. 168. Compare this with Thelma Lavine, *op. cit.*, p. 150. See also Talcott Parsons, *op. cit.*, pp. 82–83 and 5, where it is made clear that the conceptual framework, though not necessarily the technique of verification, of the theory of action is couched in terms of subjective categories.

[39] See Heinz Eulau, *Political Behavior* (Glencoe: Free Press, 1956); and his article, "Political Science" in Bert F. Hoselitz, *A Reader's Guide to the Social Sciences* (Glencoe: Free Press, 1959), pp. 89–127.

[40] Herbert McClosky, "Conservatism and Personality," this REVIEW, 52 (March, 1958), 27–45.

[41] *Ibid.*, p. 30.

From these rather vague generalizations culled from conservative literature McClosky went on to isolate an "initial pool of 43 items"—that is to say, a tentative set of fairly brief and straightforward statements embodying what he thought were elements of conservative doctrine. This entire set of items was then submitted, "through survey methods, to a large general sample of persons in the vicinity of the Twin Cities who were asked to state, in relation to each item, whether they agreed or disagreed." This was done in order to select items which clustered sufficiently "to convince us that they belonged to the same universe, . . ." to reduce the number of items, and to insure consistency among them.[42] With the twelve-item conservatism measure that emerged from the analysis of the results of this survey, McClosky found that he could rank people from extreme conservatism at one end to liberalism on the other in a manner very close to the ranking obtained using the original 43-item pool.

In addition, the procedures used to validate the scale, like those used to construct it, exemplify the methodological principle previously stated. In order to determine the degree to which the scale measured what it purported to measure, McClosky submitted subsets of items from the twelve-item conservatism scale

> . . . to an advanced class in political theory, whose members had no prior knowledge of the study or its purposes. Each student was asked to supply a name or label for the group of statements and to write a paragraph explaining or justifying the label he had chosen. Of 48 students participating, 39 volunteered the word *Conservatism* as best describing the sentiments expressed in the statements, five offered names that were virtually synonymous with conservatism (e.g., traditionalism), while two supplied other names and two did not answer.[43]

Thus, just as McClosky developed his original propositions ("items" in the questionnaire which defined the operational concept of conservatism) out of statements drawn from the traditional doctrines of conservatism, so he also validated the concept of conservatism by determining whether living persons who were presumably aware of the contemporary definitions of conservatism in the tradition of political thought and in American society shared the same concept of conservatism. Undoubtedly it would be possible for a "crude behaviorist" to claim that all that this study is based on is the assumption that a conservative is someone who is defined by a series of marks on a certain sheet of paper. "Conservatism is what conservatism tests test," one might say, echoing the well-known tautology about IQ tests. This, however, was not McClosky's assumption. His introductory remarks clearly indicate his assumption that conservatism, like liberalism, is a set of ideas that are actually efficacious in orienting human behavior and have been, and are, historically existent. Conservatism is not, that is, merely a scientific construct, a classification developed in the minds of scholars in the hope that it might serve as an index of future behavior, much as a high score on an IQ examination seems to serve as a sign of future high performance in college.

On the contrary, conservatism is primarily a complex of notions that is actually operative in political life and McClosky's "Conservatism scale" is an attempt to measure, in the sense of ranking, a number of individuals along what is assumed to be a real attitude dimension. By forming his basic concepts on the basis of the expressions of conservative ideas given him by both past and present social actors or agents who are active, at least intellectually, in political life, he has insured that his eventual explanations of political preference, belief, and affiliation are based on concepts which are meaningful to those agents as well as to the scientific observer.

Attitude studies such as McClosky's thus indicate how it is possible to form, through the development of scales, objective concepts of so-called subjective meaning-structures. In the science of politics, as in all empirical sciences, the concepts we are dealing with have intersubjective validity and are not merely introspective, and the propositions we are dealing with—though there is an element of *verstehen* in their constituent concepts—are verifiable through methods that are similar in all the sciences.[44]

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[42] *Ibid.*, pp. 30–31. Altogether, over 80 pools of items were submitted, the conservatism pool being only one of them.

[43] *Ibid.*, pp. 32–33.

[44] In addition to examining the contributions in the science of politics toward understanding the "orientations toward politics" which we have been discussing, Heinz Eulau's *Political Behavior* examines research done on the "agents and techniques of political power" and on activities going on in the "arena of political decision-making." The first is concerned with leadership studies, studies of the "mechanisms" by means of which influences are mobilized and transmitted. Often these studies are simply historical-descriptive analyses of social origins, religious affiliations, etc. See pp. 184–193. Where they are not, they are similar to the studies

Participation Studies. Another major type of inquiry is that which studies modes of political participation. For example, in a research study done a few years ago to determine how the Standard Oil Company (N.J.) stands with the American public, it became important to also find out who are most articulate in their feelings about big corporations and most concerned to attack or defend them. Since it was necessary to segregate the people who are relatively active in relation to national political issues from those who are relatively inactive, it was first necessary to define what "political activity" would mean for the purposes of this study.

While we are not interested in the content of the definition, the procedures followed in developing it are relevant. In defining "political activity" the authors first wrote out a large number of questions which they thought would throw light on an individual's political behavior, and then they tried these questions out "in personal interviews with a test national sample of American adults. Some of the questions were found not to work well in doorstep interviews, and were discarded. Others were found to yield results so closely intercorrelated that one question could in effect stand for several others."[45]

For our purposes, it is important to note that some questions were found to *mean* the same as several others, and could, in fact, be used in place of several other questions. The criteria of identity utilized in forming this concept of "political activity" were developed in terms that were as meaningful to the agents as to the observers on two levels. The first level is the level of language which is shared by agent and investigator; and, while this point is often neglected, this is what makes it possible for the scientist to "rough-up" a set of preliminary questions with the assurance that they will have at least a minimum of mutual significance and utility. But, secondly, we can also see in these studies that a conscious attempt was made to reduce the questions to the fewest which are most meaningful to the respondents, which most correspond to what they conceive of as "political activity." Aside from "face validity" (meaningfulness on the shared language level), the authors only claim that the resultant set of questions works well "in the sense that the questions are comprehended and answered with apparently little exaggeration by people of all educational levels and political persuasions."[46] What does this mean but that the investigator and the agent both understood the meaning of the questions, and that the investigator in some way felt that he knew that the agent understood?

Participation studies of this sort are relatively simple versions of the more complex studies which have come to be called studies of "community power structure." While some of these studies make use of a certain amount of participant observation, they all depend almost wholly on interviewing. Included in the interview schedules are always questions designed to measure what Herbert Simon calls "beliefs as to where legitimate power lies." In a recent study of participation in a small community, for example, "the sample was asked to nominate others as 'generally more influential,' and influential in regard to school matters, local government affairs and community welfare policies. . . ." In addition, in regard to these same issues, the respondents were asked to designate potential policy advisors by asking them: "Whom could you go to among your personal acquaintances, or persons you feel you could go to, for advice as to what should be done in regard . . ." to these issues?[47] The use of questions of this type indicates, again, that the political scientist wants and needs to develop concepts which are as meaningful to the agents as to the investigator. Answers to questions such as "Whom could you go to among your acquaintances for advice? . . ." obviously assume notions in the mind of the agent asked about the rule or norm of behavior that he is following in this context. "Though I am not now in the process of actually seeking such advice," the answer might be, "I *make it a rule* to consult X." Even in those cases in which the answer must be descriptive of past behavior—as in answer to the question, "whom have you consulted during the past year on issue Y?"—it

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under the rubric "orientations toward politics" as far as the problem of concept formation goes. See pp. 193–204, 260–264. The second group is likewise concerned with applying the same procedures, only in the context of specific (non-community wide) loci, or arenas, of influence. The subject matter, i.e., influence, or political power, remains the same, as do the procedures involved in concept formation. Some of these studies involve merely descriptive statistics (pp. 275 ff.); others illustrate the type of concept formation analyzed (pp. 286–307).

[45] For example, Julian L. Woodward and Elmo Roper, "Political Activity of American Citizens," reprinted from this REVIEW, 44 (December, 1950), 872–885, in Heinz Eulau, and others (eds.), *op. cit.*, p. 133.

[46] *Loc. cit.*

[47] Robert E. Agger and Vincent Ostrom, "Political Participation in a Small Community," in Heinz Eulau, and others, *op. cit.*, p. 139.

should be clear that the question implicitly assumes that the principle according to which X has been consulted about Y continues to hold. For if, at the time the question was asked, the agent or respondent no longer intended to consult X, the proper answer would be, "I did see X, but now I distrust his judgment and will no longer consult him." At that time, X is no longer an influential in this respect. Clearly, in no case do we actually see the agent consulting X, nor do we know that his visit is to be identified as "a consultation about policy Y" unless he tells us that that is the meaning it has for him and for X. This "meaning," since it involves concepts (normative), can only be "discovered" by having them expressed in the form of statements or propositions—which is why interviews and questionnaires are so widely used in the social sciences, and why anthropologists entering upon the study of some exotic society must learn the language and saturate themselves in the traditions and history of the society. The latter is the only way that they can understand the patterns of action which are the institutions of the society. "Patterns of activity," therefore, actually are *normative concepts* communicated to the investigators by the agents.[48] Survey research, field studies or participant observation are techniques of applying the methodological principle that behavioral concepts, or constructs of "what is to count as doing the same thing," are derived from the normative concepts held by the subjects studied.[49]

V. THE RECENT REVISION OF BEHAVIOR THEORY

The thesis we have been supporting can be placed in a larger context by considering some of the recent changes that have occurred in behavior theory proper—that is, in the psychological theory of human behavior. These changes apparently have been neglected by both the opponents and the theoretical proponents of behavioralism in the study of politics.

Charles E. Osgood, in a recent analysis of some of the major trends in psychological theory, revealed the agreement among psychologists that the basic principles of simple stimulus-response (S-R) psychology are insufficient (but not necessarily wholly invalid) for an adequate analysis of human behavior.[50] In his lucid summary of the development of behavior theory in psychology, Osgood declares that the psychologist has always thought of himself as limited to observing what goes into the human organism, or "stimuli," and what comes out of it, or "responses." Between these two points at which observation is possible, there lies what he calls a "no-man's land of speculation." This "area" is, at least for the physiologist, the nervous system; sometimes psychologists have referred to it as "the little black box" in which stimuli are focused and in which, it is assumed, responses are created. Now, according to early behavior theory

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[48] In a recent methodological study of anthropology S. F. Nadel wrote that it is invariably necessary for the anthropologist to go into the field and make himself "familiar" with the culture (usually primitive) that he wishes to study and overcome its strangeness by "something like an intellectual assimilation." Significantly, he stresses the fact that the study of the native language will probably have to precede the study of culture and social life. Since the meaning of terms and sentences of this new language cannot be gotten by counting the letters or words "scientifically" but must be *learned,* Nadel is obviously implying that before any of the "objective" controls used by anthropologists can be applied the investigator must learn the meaning of the concepts, and therefore of the institutions, of the new culture. S. F. Nadel, *The Foundations of Social Anthropology* (Glencoe: Free Press, 1951), p. 6. Underlining added.

It is also relevant to note the criteria Nadel suggests for isolating different types of so-called action patterns. "We have long held," he states, "that the order of things to which social institutions belong is built up through collecting together standardized action patterns *on the grounds of their aim contents*. . . . It might also seem that the people we observe themselves have names for these 'summaries' of *related action patterns*—'marriage,' 'family,' 'chieftanship,' 'property,' and the like. All such names . . . *stand for normative concepts;* the institution represents, for the actor, a rule or norm, *and has that kind of reality, that is, the non-spatial and in a sense timeless validity of concepts." Ibid.,* p. 107. Underlining added.

[49] In addition to Peter Winch, *loc. cit.,* see Michael Argyle, *The Scientific Study of Social Behavior* (London: Methuen, 1957), pp. 14–26, where it is pointed out that the social scientist has two general types of measuring instruments all his own, namely, the different kinds of interview and questionnaire, and the techniques of controlled observation (the types we have discussed), and where it is also stated that investigators must be ". . . well steeped in the culture of the group members, . . ." to insure the correctness of their interpretations of the results of their tests and observations.

[50] Charles E. Osgood, "Behavior Theory and the Social Sciences," *Behavioral Science,* 1 (July, 1956), 167–185. Reprinted in Roland Young (ed.), *op. cit.,* pp. 217–244.

> . . . the psychologist's task is to explain and predict relations between and among his two sets of observables, stimuli and responses, and to do this he must make certain assumptions about what goes on in the little black box. In other words, psychological theory, as contrasted with psychological observation, is made up of hunches about how the nervous system operates.[51]

Over the last century, however, psychologists found that a variety of phenomena appeared again and again in the midst of their traditional inquiries which proved embarrassing to the old single-stage (S-R) conception of behavior. These were symbolic phenomena, or "representational mediation processes." In their attempts to come to terms with these phenomena, Osgood points out that psychologists have produced a slow refinement in the rules of procedure by which they made and tested their "hunches" about what went on in the "little black box." His description of these developments is interesting, amusing, and succinct:

> In nineteenth-century psychology the characteristic procedure in theorizing was to simply postulate a new entity or mechanism whenever some new regularity was discovered. Whenever something needed explaining, a new explanatory device was stuck inside the little black box, and it rapidly became chock-full of ill-assorted and ill-digested demons. For every nameable phenomenon of human behavior a different 'faculty' would be posited to explain it; for every nameable motive, a different 'instinct' would be listed as its explanation. And . . . Freud had big, flat-footed Super egos stomping around on red slippery Ids, while cleverly anxious little Egos tried to arbitrate. Thus . . . the little black box was filled with a wonderously diverse collection of explanatory devices, just about as many as there were things to be explained. This could fairly be called 'junk-shop psychology.'
>
> In direct revulsion against this brand of theorizing, a group of American Behaviorists around the turn of the century went to the other extreme, claiming that the psychologist was better off if he made no assumptions whatsoever about what went on in the little black box . . . . This viewpoint toward theory has come to be known as 'empty organism psychology.' According to this view . . . there is absolutely nothing in the region between S and R, and what *is* there is none of the psychologist's business![52]

Gradually it became necessary for psychologists to postulate some sort of intervening variable between S and R. They had to put something back into the "little black box," as it were, in order to explain their observations. The contemporary behaviorist therefore has postulated an intervening or internal response-like process which produces self-stimulation. In other words, ". . . modern 'mediation psychology' sets up within the organism a replica of the S-R model, and it assumes that the same laws governing single-stage S-R process apply to both stages of the mediation model."[53]

Osgood goes on to develop much more complicated models of this mediation process, but the point remains the same. The observed behavior not only of humans, but also of animals, suggests that it is necessary to assume that often external stimuli arouse some intervening process which in turn produces a certain behavior—"problem solving," "learning," etc.,—which is partially independent of the stimulus. Osgood mentions several facts in both animal and human behavior which cannot be handled with a single-stage S-R model.[54] The term generally used to refer to this intervening process is "symbolic mechanism"—but, though the phrase seems Hobbesian, the methodology appropriate to a study of symbolic behavior indicates that a mechanistic analogy is inappropriate. As Osgood states, this type of theory

> . . . places a premium on developing methods of indexing representational states, e.g., measuring *meaning,* particularly in human organisms. My own research . . . has been directed chiefly along these lines. Dealing with language responses themselves—which, after all, are supposed to be 'expressions of meaning'—we have been trying to discover a limited number of basic factors or dimensions along which meaningful reactions vary and hence can be measured.[55]

It seems to me that in "dealing with language responses" Osgood—and other modern behaviorists dealing with somewhat different problems—must operate in a context of shared meaning with their "respondents" or "subjects." That this is the case is indicated by an example given in Osgood's article of a study of abstract "assigns," that is to say, symbols or terms whose meanings are literally assigned through repeated association with simpler signs having direct behavioral reference.[56] This implies that meanings can have an existence independent of immediate physical stimuli. As Osgood points out, it is precisely this which makes possible imitation, identification, and the sharing of personality and culture traits. Furthermore, it is precisely this learning process that lies at the bottom of all types of social behavior. And it is this capacity of personality to construct "equivalences" (which refer to the same fact that Peter Winch called the need and the ability to formulate rules) which lies at the bottom of concept formation in everyday life as well as in the professional life of the behavioral scientist. Thus, the philosophical objection of Peter Winch to behaviorism—that the social scientist must base his judgments of identity, his classifications, and therefore his entire conceptual scheme on the judgments of identity (or the meaning) attributed to their own and others' behavior by the subjects studied themselves—is a valid criticism of what Osgood has called the "single-stage S-R process" or early behaviorism, but it provides only a philosophical clarification of modern behavior theory.

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[51] *Ibid.,* pp. 217-218.

[52] *Ibid.,* pp. 219-220.

[53] *Ibid.,* p. 220. See also Arnold Rose, *Theory and Method in the Social Sciences* (Minneapolis: University of Minnesota Press, 1954), pp. 157, 275–278.

[54] Osgood, *op. cit.,* p. 233. See also Felix Kaufmann, *op. cit.,* pp. 154–55; Ernest Nagel, *The Structure of Science,* p. 477.

[55] Osgood, *op. cit.,* p. 239.

[56] *Loc. cit.*

Thus, both the present practices of behavioral students of politics and contemporary behavioral theory have converged toward a rediscovery of mind in the sense of assuming representational states whose meaning is expressed by language responses. No longer is mind nothing but a "subjective entity" to be discarded from scientific inquiry according to the program of Bentley and the "radical behaviorists." And the techniques which have been developed to rank individuals along attitude dimensions according to their meaningful statements (not, it should be noted, to "measure" the concepts in their heads) do not merely apply the methods of the physical sciences to the "moral sciences" as Mill and his intellectual descendents hoped would be possible. No physical scientist is ever required in his studies to ask questions of his objects of inquiry as part of the process of concept formation; yet, this has become in practice and in theory a fundamental principle of behavioral research.

VI. CONCLUSION

The notion that it is possible and sufficient to observe overt patterns of social interaction in the same way it is possible to observe the pattern of tracks left in the snow by some winter stroller belongs to the doctrines of early behaviorism as exemplified in political theory in the writings of Bentley. Although it is no longer present in either the theory or practice of behavioral science, it remains a vestigial feature of the idea of a science of politics in the minds of some of its critics. These critics have advocated the thesis that the study of politics cannot eliminate common sense notions of events and behavior and the distinctions made in ordinary language.

However, a critical understanding of the notion of *verstehen,* which has been put forth as the method of discovering common-sense meanings, led to the conclusion that indeed the political scientist must base his judgments of identity, and therefore his classifications, on the judgments of identity (or the meaning) attributed to their own and others' behavior by the subjects or agents who are being studied. Participants in the social situation itself "define the situation," that is, define the underlying norms guiding action, and therefore also the institutions and "patterns of behavior" that the political scientist investigates. But we also concluded that developing criteria of identity and forming concepts is a problem of semantics, and thus a philosophical problem. Therefore, our procedures at this level of rational activity do not change the fact that, once those concepts have been formed and made use of in the construction of scientific hypotheses, the procedures or methods for *verifying hypotheses* are logically the same in the social as in the physical sciences.[57]

The most urgent question of this part of the

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[57] We may agree with Nagel and with Schutz that all of the empirical sciences must be objective "in the sense that their propositions are subjected to controlled verifications and must not refer to private uncontrollable experience." Alfred Schutz, *op. cit.,* p. 270. We may also agree that in the empirical sciences, theory means "the explicit formulation of determinate relations between a set of variables in terms of which a fairly extensive class of empirically ascertainable regularities can be explained . . ." (and that) "neither the fact that these regularities have in the social sciences a rather narrowly restricted universality, nor the fact that they permit prediction only to a rather limited extent, constitutes a basic difference between the social and the natural sciences, since many branches of the latter show the same features." *Ibid.,* pp. 260–261. See also Ernest Nagel, *The Structure of Science,* pp. 484–485.

See also Donald Davidson's defense of the notion that explanation by motives is a species of causal explanation rather than incompatible with it: *The Journal of Philosophy,* 60 (November 7, 1963), 685–700.

A careful analysis of the various meanings of "meaning" and "understanding" which makes clear how a scientist can "understand" say, religion, as an observer without "understanding" it as a believer is presented by May Brodbeck in "Mean-methodology of political science then becomes: "How is it possible to form objective concepts and an objectively verifiable theory of subjective meaning-structures?"[58] How this is possible, as well as evidence for the fact that modern behavioral scientists actually do form their concepts on the basis of concepts that are meaningful to the agents being studied, was illustrated by examining some recent behavioral research.[59]

In this sense, the foregoing criticism of the notion of *verstehen* together with the analysis of the procedures actually involved in developing criteria of identity provide a theoretical clarification of some of the methodological assumptions underlying what modern behaviorists are actually doing in their empirical research. Likewise, political theorists, traditionalists, or "anti-behavioralists" should realize that "under the pressure of trying to handle language phenomena," scholars working in behavioral science have modified their postulates and "have found it necessary to put something else back into the little black box." The "little black box," the human organism, is no longer postulated as an empty cipher. "Central integration," as well as "symbolic, representational processes," are necessary assumptions to explain language and other behavior. And it should be obvious to the "anti-behavioralists" that a discipline which assumes, again in Osgood's words, that "language responses are indicative of meaning," and that methodologically the psycho-social researcher must deal with those language responses themselves to determine "the limited number of dimensions along which meaningful reactions vary,"[60] is making an assumption about "human nature," and about language and reason, that is no different from the assumption which traditional political theorists make about the meaningfulness or intentionality of political life.

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ing and Action," *Philosophy of Science*, 30 (1963), 309–324. Professor Brodbeck also argues our thesis that participation in a culture is necessary to achieve *both* these (and other) kinds of "understanding"; yet that the use of a common language for communication is not incompatible with the viewpoint of the scientific observer. Both articles are reprinted in May Brodbeck (ed.), *Readings in the Philosophy of the Social Sciences* (New York: Macmillan Co., 1968), pp. 44–78.

[58] Alfred Schutz, *op. cit.*, p. 270.

[59] A difficult problem arises, however, as soon as this viewpoint is accepted. That is the problem created by the possibility that an actor might himself be wrong regarding what he is doing or intending to do. Neurosis, obtuseness, malice, lack of knowledge regarding possible unintended consequences all make it difficult for the social scientist to equate an individual's expressed intention with the "real meaning" of his act. On the surface it would seem that modern research technology does cope with this problem through proper questions and analysis of answers. On the other hand, the problem of unintended consequences especially would seem to push one on from the level of concept formation to the level of explanation. In either case, the problem seems sufficiently complex and related to other aspects of theory construction to warrant separate investigation.

[60] Charles E. Osgood, *op. cit.*, pp. 221–222.

DECISION-RULES AND INDIVIDUAL VALUES IN CONSTITUTIONAL CHOICE*

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Once a political community has decided which of its members are to participate directly in the making of collective policy, an important question remains: "How many of them must agree before a policy is imposed on the community?" Only if participation is limited to one man does this question become trivial. And this choice of *decision-rules* may seem only a little less important than the choice of rules[1] in a world so largely governed by committees, councils, conventions, and legislatures. This paper is about the consequences of these rules for individual values.

Both the oral and written traditions of political theory have generally confined the search for optimal (or "best") decision-rules to three alternatives.[2] The *rule of consensus* tells us that all direct participants must agree on a policy which is to be imposed. *Majority-rule* tells us that more than half must concur in a policy if it is to be imposed. And the *rule of individual initiative* (as we may call it), holds that a policy is imposed when any single participant approves of it. These three decision-rules—"everyone," "most of us," and "anyone"—are terribly important, but they cannot be said to exhaust the available alternatives.

The list of alternatives is just as long as a committee's roster. Only for a committee of three would 'consensus,' 'majority' and 'individual initiative' exhaust the possibilities. In a committee of n members, we have n possible rules. Let the decision-rule be a minimum number of individuals (k) required to impose a policy. For a committee of five, we have five alternative values of k:

individual initiative		majority rule		rule of consensus
↓		↓		↓
$k=1$	$k=2$	$k=3$	$k=4$	$k=5$

($n=5$)

This means, of course, that the number of possible rules is often very large. The U.S. House of Representatives might, for example, choose any of 435 decision-rules; the British House of Commons could choose any of 630 rules; and the Supreme Soviet might choose any of 1300 rules. A committee of n can, in short, choose any of n possible rules.[3] And one can hardly claim to have identified the 'best' decision-rule, whatever his normative standard, unless he has shown it superior to the n−1 alternatives.

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* An earlier version of this paper was read at the International Conference on the Mathematical Theory of Committees and Elections, Vienna, June, 1968. For helpful criticisms of that earlier version, I thank Jay Casper, Richard Curtis, Robert Dahl, William Riker, Arnold Rogow, and Michael Taylor.

[1] Well, maybe somewhat more than a "little less important," but not trivial by comparison: for evidence in a very practical argument, consider J. J. Servan-Schreiber, *The American Challenge* (New York: Atheneum, 1968), pp. 170–178.

[2] This limitation is illustrated by Abraham Lincoln's remark: "Unanimity is impossible; the rule of a minority as a permanent arrangement, is wholly inadmissable; so that, rejecting the majority principle, anarchy or despotism in some form is all that is left." Cited in Henry B. Mayo, *An Introduction to Democratic Theory* (New York: Oxford University Press, 1960), p. 178, and in Thomas Landon Thorson, *The Logic of Democracy* (New York: Hold, Rinehart and Winston, 1962), p. 142.

[3] Rules with a value equal to or less than n over 2, of course, present special liabilities, for they permit the formation of more than one winning coalition on a single issue. But for some classes of issues this is not a serious difficulty, since multiple policies may be effected. An example is provided by *certiorari* proceedings in the U.S. Supreme Court: in these instances, the so-called "rule of four" ($k = 4$, $n = 9$) is applied, so that any four justices may require that a writ be granted. This produces no difficulties, since the granting of one writ does not preclude the granting of another—multiple policies are permissible.

This paper reports one normative analysis of the decision-rules available to a generic political committee.[4] In it, I consider a single, anonymous individual who wishes to optimize the correspondence between his own values, however selfish or altruistic, and those expressed by collective policy. This individual would like to "have his way" as often as possible, by securing the adoption of proposals he likes and the defeat of proposals he dislikes. For him, then, the "optimal" decision-rule will be the one which minimizes the frequency with which he must expect to support a proposal yet have it defeated, or to oppose a proposal yet have it adopted. This is because his values will correspond to collective policy most closely under rules which minimize the frequency with which he is "outvoted" in one of these two ways. And, for him at least, the best decision-rule will be the one which minimizes the expected frequency for these unhappy events. The resulting normative criterion is elaborated in Part I below.

The formal analysis begins (in Part II) with a simple model for the operation of political committees. This is a "null" model in the sense that it presumes very little information about the future. Under it, we know nothing about the (long-run) agenda which will confront the committee, about the ways individuals will evaluate the proposals which do arise, or about the factional structure of the committee. This model serves two purposes. First, it produces a recommendation appropriate to the circumstances of "constitutional choice": If we presume to know very little about an indefinite future, this model rigorously demonstrates the optimality of majority-rule under our individualist criterion. And second, the results of the simple model provide a "baseline" against which to consider the normative implications of additional information about the future. In Part III, we consider the consequences of information which would, for example, lead us to expect the formation of a standing minority faction, or a general prejudice for or against government action.

Let me begin by explaining my normative criterion and distinguishing it from three leading arguments about decision-rules—those which rest on the ideals of equality, economy, and "truth."

I. A NORMATIVE CRITERION

Let us suppose that the individual with whom we are concerned has some personal schedule of values. We know nothing about the specific content of his value schedule—its selfishness or altruism, its sophistication or naivete, its generality or specificity—since this individual, whom we may call "Ego," is a purely generic figure. We must, however, suppose that his schedule is complete enough that it leads Ego to like or dislike each proposal which comes before the committee. Accordingly, or major assertion is that Ego would like to optimize the correspondence between his preferences and the policies which are imposed by the committee:[5] he wants to have his way by defeating proposals which his values lead him to dislike and by imposing those which they lead him to like. Our major normative assumption is then:

> *Value Assumption:*[6] Ego wishes to optimize the correspondence between his schedule of values and the list of policies which are imposed.

Now let us suppose that Ego looks ahead and contemplates four events which might occur:

A. Ego's value schedule leads him to support a proposal, but it is not imposed.

B. Ego's value schedule leads him to oppose a proposal, but it is imposed.

C. Ego's value schedule leads him to support a proposal, and it is imposed.

D. Ego's value schedule leads him to oppose a proposal, and it is not imposed.

Ego need not be much of a logician to see that our "Value Assumption" will be best served by the frequent occurrence of events C and D. Since it is also clear that the four classes of events are disjoint, Ego can be sure that a decision-rule which minimizes the summed frequencies of A and B will also maximize the summed frequencies of C and D. Hence, Ego can see that the lower the frequency of events A and B, the higher the frequency of C and D, and the higher the degree of correspondence between his schedule of values and the list of policies imposed by the committee. This leads to a criterion by which Ego may evaluate decision-rules.

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[4] The term "political committee" is used here in its broadest sense—to include all bodies which vote on collective policies. This use is illustrated by Duncan Black's book, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1963).

[5] This assumption is less innocuous than it may appear: complete indifference is not improbable even among the very active (viz. committee members).

[6] Here, I am putting aside the theoretically important "problem of intensity." In effect, the value assumption implies that all issues evoke equally intense preferences. This decision rests on theoretical intractability of the problem. Some of the major difficulties are outlined in, Willmoore Kendall and George Carey, "The 'Intensity' Problem and Democratic Theory," this REVIEW, 62 (1968), 5–24, and in Douglas W. Rae and Michael Taylor, "Some Ambiguities in the Concept of 'Intensity,'" *Polity,* 1 (March, 1969).

Criterion for Choice Between Decision-Rules: One should choose that decision-rule which minimizes the sum of the expected frequencies for (A) in which the committee does not impose a policy which has value schedule leads him to support, and (B) in which the committee imposes a policy which his value schedule leads him to oppose.

This is the normative criterion which underlies our analysis. Each decision-rule will be evaluated by the frequency with which we must, under given assumptions, expect these events to occur if that decision rule is adopted. Our initial commendation of majority rule will imply nothing more than this: under majority rule, the expected (summed) frequency of events A and B is at a minimum, and majority rule is therefore optimal under this criterion. This in turn suggests simply that majority rule maximizes the probability that our (anonymous) individual will "have his way" with respect to a given proposal. And this, in the long run, suggests that majority rule will optimize the correspondence between individual values and collective policies. Since "having one's way" is its central mechanism, we may call this criterion *political individualism.*

The "moral tone" of this position is exactly that of the individuals with whom we are dealing—no better and no worse. Under the criterion of political individualism, we must recommend decision-rules which optimize the correspondence between individual values and collective policies, even if we do not like these values, and even if we do not consider these values "rational" in some larger context.

The best known alternative to this criterion is also independent of the "morality" or "rationality" of individual values: this is, of course, the criterion of *political equality.* While "equality" is a word of many meanings, its most relevant use for present purposes is the rule that, ". . . the weight assigned to the choice of each individual is identical."[7] A minimum consequence of this criterion is that all decision-rules must be defined by the *number* of members who must vote yes if a policy is to be imposed, regardless of *which* members these may happen to be.[8] This criterion is the best-known basis for recommendations of majority-rule. As Neal Reimer puts the case, any alternative rule amounts ". . . to ignoring the majority: to declaring that the minority shall be considered and weighted *unequally.*"[9] In the present analysis, this is taken for granted: we are equally concerned with each individual member, and we define decision-rules without respect to the identities of the individuals voting yes and no. Here we are examining the additional requirement that a decision-rule optimize the expected correspondence between these (equally valued) individual preferences and the policies which a committee adopts.

A second and more restrictive alternative to political individualism is offered by the family of criteria based on the *truth-value* of collective policies. An example is provided by the 15th Century Council of Ferrara, which adopted a two-thirds decision-rule for proposals to alter Church doctrine, apparently on the presumption that this was a hedge against theologically fallacious changes.[10] Another example is held out be Rousseau's first criterion for the choice of a decision-rule: ". . . the more grave and important the questions discussed, the nearer should the opinion that is to prevail approach unanimity."[11] The apparent presumption is that voting is a sort of sampling device for the determination of the General Will: restrictive sampling criteria reduce the chance of a policy inconsistent with the General Will being adopted. Our criterion makes no claim to the optimization of truth values, mainly because such a criterion seems epistemologically indefensible, and because it suggests that past policy is necessarily "truthful," independent of changing circumstances.

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[7] See Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), pp. 67, 84.

[8] This requirement is closely related to K. O. May's condition of "anonymity," under which the decision-rule does not depend in any way upon the identification of individual members. See his "A Set of Independent Necessary and Sufficient Conditions for Simple Majority Decision," *Econometrica,* 20 (1952), 680–684. May's paper shows that majority rule alone satisfies "anonymity" and three other conditions simultaneously.

[9] Neal Reimer, "The Case for Bare Majority Rule," *Ethics,* 62 (1951-52), 16–32. The cited statement is from p. 17.

[10] Thomas Baty, "The History of Majority Rule," *The Quarterly Review,* 216 (1912), 1–28 See especially pp. 22–23. The Council also was tri-cameral: within each chamber, a two-thirds rule prevailed; between chambers the rule of consensus was followed. This is indeed a restrictive decision-rule, and a fine hedge against doctrinal error, *if* we assume the existing doctrine is altogether correct.

[11] *The Social Contract,* translated by G. D. H. Cole (New York: E. P. Dutton, 1950), p. 107. Rousseau's second rule was that ". . . the more the matter in hand calls for speed, the smaller the prescribed difference in the number of votes may be allowed to become: where an instant decision has to be reached, a majority of one should

Yet a third alternative, much closer to the present criterion, is offered by *economic individualism*. This criterion is embedded in the often fruitful application of economic methods to politics—the works of Olson, and Buchanan and Tullock being important examples.[12] As the latter authors put the criterion, ". . . individuals will, on the average, choose more rather than less when confronted with the opportunity for choice in the political process, with more and less being defined in terms of measurable economic position."[13] In their analysis of decision-rules, Buchanan and Tullock define two cost functions: (1) "external costs," which are defined by the costs a single individual ". . . expects to endure as a result of the actions of others," and (2) "decision-making costs," which are defined by "the time and effort . . . which is required to secure agreement . . ." among the number of people required by a decision-rule to impose a policy.[14] One then looks for a rule which minimizes the sum of these costs.[15]

While this approach is in many respects a powerful one, I have chosen not to follow it here for two major reasons. The first is most important: we are interested in a political problem in its own right, and this must not be submerged in the conceptual (sometimes even ideological) bath of economic individualism. We do not wish, therefore, to conceive the problem of having one's way as a means to success in a larger economic game, but to leave the source of individual values open to the unspecified "value schedules" of individuals. A second, less important reason is methodological: I know of no way to associate costs with decision-rules and at the same time assure that these costs are additive. So, instead, I have chosen to examine expected frequencies for having one's way without subsuming these frequencies under more general cost-functions. While this analysis owes important intellectual debts to these economic models—especially to Buchanan and Tullock—it should not be viewed as a contribution to that tradition.[16]

We may now proceed with our analysis, taking political individualism as our normative criterion. We begin with an individual who tries to evaluate decision-rules under the bewildering uncertainty of "constitutional choice."

II. A SIMPLE CONSTITUTIONAL CHOICE MODEL

We are now interested in the frequencies with which our generic number (Ego) must expect the events A and B to occur under a given decision-rule. We begin, here, with a model which generates these expected frequencies from the temporal (and epistemological) perspective of "constitutional choice." As James Buchanan ex-

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be enough." Condorset actually devised a truth-function theory using probabilistic assumptions similar to those found in Part II below. See *Essai sur l'application de l'analyse à la Probabilité des Décisions Rendues à la Pluralité des Voix* (Paris, 1785).

[12] Mancur Olson, *The Logic of Collective Action* (Cambridge, Massachusetts: Harvard University Press, 1965); and James Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962).

[13] Buchanan and Tullock, *op. cit.,* p. 29. The quotation is one of two interpretations which these authors give to the more general criterion of utility maximization.

[14] *Ibid.,* pp. 64, 68.

[15] *Ibid.,* p. 70. "For a given activity the fully rational individual, at the time of constitutional choice, will try to choose that decision-making rule which will *minimize* the present value of the expected costs that he must suffer. He will do so by minimizing the *sum* of the expected external costs and expected decision-making costs. . . ." It is not clear that these costs can be added on any single scale. For a commentary on these cost functions see, Herbert J. Kiesling, "Potential Costs of Alternative Decision-Making Rules," *Public Choice*, 4 (1968), 49–66.

[16] Although Buchanan and Tullock produce no rigorous evaluations of decision-rules, two of their speculations seem especially relevant to the present analysis. First, they point out that majority-rule holds no special *a priori* promise as a cost minimization (p. 81). Second, they suggest that ". . . the low-cost point on the aggregate 'cost curve' would tend to be represented by simple majority voting," under circumstances which appear comparable to those presented in Part II of this paper (p. 129). No strict demonstration of this tendency is offered, and care must be taken not to confuse the frequency curves employed here with the cost-functions which these authors consider. If any determinate relation exists, and I am not sure one does, it is between the frequencies usd here and "external costs" in their analysis.

plains, this perspective drastically limits the information which may be incorporated in our model:[17]

> The center of attention becomes the mental calculus of the individual as he is confronted with a choice among alternative rules for reaching subsequent political decisions. The individual does not know, nor is he able to predict, what particular issues will be presented subsequent to the adoption of the rule. And, even if he [could] predict with some accuracy the sort of issues that may arise, he could hardly predict his own position vis-a-vis the other members of the group.

The model presented here assumes no knowledge of an indefinite agenda and the preferences of individuals at discrete points over that agenda

The more formal model developed below rests on six rather concrete assertions which may be stated at the outset:

1. We are dealing with a political committee, that is, one which is authorized to make collective policies.
2. This committee will deal with a succession of policy proposals, and is free to impose or reject each.
3. This list of proposals (agenda) is unknown at the time the decision-rule is chosen.
4. Since the proposals are unknown, so are the preferences of the members on each proposal.
5. The committee will contain n coequal members, and n is at least three $(n \geq 3)$.
6. Each member either supports or opposes each proposal.

We suppose that one member of this committee, whom we call Ego, is interested in making a choice among the available decision-rules. His choice may be established by a means-ends inference from: (1) the normative criterion developed above, and (2) the model developed here for the purpose of predicting the frequencies of events A and B under each of the available decision-rules.

Our model assigns expected frequencies to events A and B, and associates these with each value of k in a committee of n members. This is accomplished by making certain assumptions about the way individual committee members decide whether to support or oppose each proposal. These assumptions allow us to generate a distribution of "support sets" for policy proposals, and these sets may be partitioned by two criteria: (1) their size (do they equal or exceed k?) and (2) their inclusiveness (how many of them of any size will include or exclude our generalized member, Ego?). These partitions are used to define "sets of support sets" corresponding to events A and B. We are then able to generate expected frequencies for these sets—that is, for the occurrence of events A and B at any point in the committee's agenda. This allows us to compare the expected frequencies of these undesirable events for each available decision-rule (i.e., each value of k), and to apply our criterion of choice to these expectations. Let us begin with our assumptions about individual decision-making by committee members.

Assumptions about Individual Decisions. As we have already indicated, each member either supports or opposes each proposal as it occurs. We know nothing of the proposals (or the member's reactions to them), so we must make certain assumptions about the ways in which members decide whether to support or oppose each proposal. Briefly, we assume that each member decides whether to support or oppose each proposal independently, on the basis of his own value schedule, and that he is exactly as likely as not to support each proposal.

Assumption I: Each member decides whether to support or oppose each proposal on the basis of his own value schedule. Value schedules are independently constructed. Therefore, the probability that one member will support (or oppose) a proposal is independent of that probability for any other member.

Let: P represent probability
E represent the eventuality that member (E) supports a proposal
e represent the eventuality that member (E) opposes a proposal
X,x represent these eventualities for member (X)
$[a/b]$ should be read "a, given b"

Then:

$$P[{}_E/X] = P_E$$
$$P[{}_E/x] = P_E$$
$$P[{}_e/x] = P_e$$
$$P[{}_e/X] = P_e$$

Since "Ego" is a generalized member, Assumption I was already implicit in our Value Assumption (above). Every member is trying to optimize the correspondence between the list of policies imposed and his own, independently constructed value schedule. It follows that the decisions of members are independent events, whose probabilities are not causally related to each other. This assumption is not, of course, meant as a descriptive generalization suggesting that members never influence or lead each other: it is simply a model assumption.

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[17] "An Individualistic Theory of Political Process," in David Easton (ed.), *Varieties of Political Theory* (Englewood Cliffs: Prentice-Hall, 1966), pp. 25–37, quotation from p. 29. For a related statement, see also Elinor Ostrom, "Constitutional Decision-Making," unpublished paper.

Assumption II: The probability that each member will support any proposal is exactly one-half, and, since he must either support or oppose each proposal, the probability that he will oppose any proposal is also exactly one-half.

Let: the notation remain the same.
Then:

$$P_E = ½ = .5$$
$$P_e = 1 - P_E = 1 - .5 = .5$$

This is arbitrary, but there are three reasons not to make any assumption different from Assumption II. First, no other assumption seems more "realistic" in an intuitive sense, and some seem a good deal less "realistic." That, however, is not a major concern. Second, any asymmetrical assumption would enormously complicate our model and its predictions because it would deny us the use of the very simple calculus of probabilities for combinations of equally likely, independent events. Third, and crucially, it can be shown that no alternative to Assumption II alters the location of the minimum value for the expected summed frequencies of events A and B. Our finding is general to the whole set of alternative assumptions. This is demonstrated in the appendix. So, in the absence of any expected alteration in our major prediction, there seems no reason to complicate the model.

Expected Frequencies of Support Sets. All proposals will evoke a response from each member, and, taken collectively, these responses will form a combination. We need to know the number of different combinations with various properties, and the probability for each. One essentially trivial assumption must be added before these calculations are begun:

Assumption III: Proposals do not enter the committee's agenda unless at least one member supports each of them. Therefore, there will be no combinations of rsponses in which no member supports the proposal.

Let: [n letters] form a combination of responses to a proposal
Other notation remains as above.

Then:

$$P_{[exyz\ \dots\ n]} = 0$$

This assumption, which has no substantial effect on our major prediction,[18] is simply our way to account for "non-events" in which no member makes a proposal. Given these three assumptions (and elementary probability theory), two important deductions appear:

Deduction I:[19] The total number of possible discrete combinations of responses for the committee of n members will be two to the n^th^ power, minus one:

Let: n represent the number of members
N represent the number of possible combinations of responses to any proposal

Then:

$$N = 2^n - 1$$

For example, a committee of three generates seven possible response combinations:

XYZ
XYz
XyZ
xYZ
Xyz
xYz
xyZ
And, $2^3 - 1 = 8 - 1 = 7$

Our strategy will be to determine what proportion of this number of possible response combinations (later called support sets) are equivalent to events A and B. This, with the help of Deducation II, will allow us to arrive at predicted frequencies for these events under various decision-rules. In passing, it is worth noting that the number of possible combinations expands very rapidly:

n	N
3	7
10	1,023
20	1,048,575
100	$12{,}676 \times 10^{26}$ apx

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[18] The effect of this assumption is to discard an event in which every member "has his way" since all would vote no (i.e., Event D.) This simply makes the prediction slightly less optimistic than it would otherwise have been, but it has no effect on the location of the A + B minimum in which we are interested.

[19] This follows from the rule of combinations: the number of combinations of size f which may be drawn from n objects equals $n!/f!(n-f)!$. If we allow f to take all values $0\ \dots\ n$, the resulting sum is 2^n. Assumption III eliminates the one case in which $f = 0$ (i.e. $n!/n! = 1$), so the result is $2^n - 1$.

This means, of course, that any effort to literally "count" response combinations (or support sets) of various types will be quite futile.

Deduction II:[20] Every possible response combination is equally probable, and that probability is always the reciprocal of the number of possible combinations for a committee of n members.

Let: $n=2$

The notation remains as above.

Then:

$$P_{XE} = P_{Xe} = P_{eX} = \frac{1}{2^n - 1} = \frac{1}{2^2 - 1} = \frac{1}{3}$$

Since this deduction is general to all values of n, it may, for example, be calculated that the probability for each combination in a committee of ten is one over 1,023.

Now, we may convert what we have been calling "response combinations" into what we will call *support sets,* which contain only the members of any combination who support a proposal. This differs from a conbination only in the matter of notation. We drop those who oppose a policy, so response combination XYz becomes support set XY. This does not mean that opposition is ignored, since (Z)'s absence here tells us of his decision quite as much as the inclusion of a small "z" would. And, since no transformation has been made, we can be sure that what we have deduced for response combinations will hold also for support sets. This is so because every response combination corresponds to one and only one support set:

Response Combination	*Support Set*
XYZ	XYZ
XYz	XY
XyZ	XZ
xYZ	YZ
Xyz	X
xYz	Y
xyZ	Z

Since assumption III eliminates uniformly negative combinations (i.e., xyz), this is general and no empty support sets occur.

Expected Frequencies for Sets of Support Sets Corresponding to Events A and B. We may now define events A and B by the "sets of sets"—collections of support sets—to which each corresponds, and compute the expected frequency of each. (A) Event A occurs when our generalized member (Ego) supports a proposal (belongs to a support set) but is unable to impose it (the size of the set is less than the decision-rule value, k). We may identify the set of support sets (A) which corresponds to event A:

Let: A be the set of support sets which defines the occurrence of event A.
k be the minimum number of members required by the decision-rule to impose a policy.
S be a support set.
S_f be a support set of size f.
$\in$ should be read "lies in"
$\approx$ should be read "is equivalent to"

Then:

$$A \approx [S_f, \text{ such that, } f < k, \ \& \text{ Ego} \in S]$$

This says simply that the set of sets (A) which corresponds to event A contains all those support sets of sizes (f) less than the minimum value imposed under a decision-rule (k), which also include our generalized member. In short, Ego fails to impose a policy which his value schedule leads him to support (event A).

We are now interested in finding out how many support sets belong to set of sets A. This will depend on the decision-rule value (k) for each committee of n members. First, it is apparent that whenever the value of k is one (i.e., the rule of individual initiative), A must be an empty set. Under rule $k = 1$, the mere inclusion of our generalized individual defines the set as $\geq k$ and means that f cannot have a value of less than k. In short:

If $k=1$, A empty

This means that the rule of individual initiative prevents any member from having a proposal he supports defeated (event A). This, however, is not much help with the whole range of values for k which exceed one. A more general formula may be used to determine the number of support sets belonging to A for all values of k:[21]

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[20] This follows from Assumptions I and II. The combinations are compounded of independent, equally probable events (i.e., individual decisions), and are therefore themselves equally probable events. Since there are $2^n - 1$ combinations (Deduction I), and each is equally probable, each has a probability of $1/2^n - 1$

[21] This formula is derived from the rule of combinations and our set definitions. Let us begin with the rule of combinations: The number of support sets of size f is:

$$\frac{n!}{f!(n - f)!}$$

Now, we wish to find the number of combinations of any size (f) which contain a generalized member (Ego), in accordance with our set definition.

Let: N_A be the number of support sets in A
Other notation remains as above.

Then:

$$N_A = \sum_{f=1}^{k-1} \frac{(n-1)!}{(f-1)!(n-f)!}$$

This formula is designed to "count" the support sets of size (f) which contain one generalized member (Ego), and to sum those for every value of f up to $k-1$. This tells us how many distinct support sets include Ego and still don't win under any rule k. By dividing by the total number of possible support sets—winning, losing, with Ego or without Ego—we may generalize this formula to the expected frequency of Event A:

Expectation for Event A: Any member of a political committee of size n, operating under decision-rule k, must expect that a proposal he supports will be defeated in:

$$\frac{N_A}{2^n - 1}$$

proportion of the instances in which a proposal comes to a vote. The full formula may be stated:

Let: E_A represent the expected frequency of event A

Other notation as above.

Then:

$$E_A = \frac{\sum_{f=1}^{k-1} \frac{(n-1)!}{(f-1)!(n-f)!}}{2^n - 1}$$

This statement is fully general to committees of any size, and it applies equally to each member of any such committee.

Table 1 gives the expected frequency for event A (E_A) for committees for three to twelve members, and decision rules from 1 to n. We

TABLE 1. EXPECTED FREQUENCIES FOR EVENT A (E_A)
(REJECTION OF SUPPORTED POLICY)*

k \ n	3	4	5	6	7	8	9	10	11	12
1	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00
2	$\underline{.14}$	.07	.03	.02	.01	.00+	.00+	.00+	.00+	.00+
3	.43	$\underline{.27}$	$\underline{.16}$	.10	.06	.03	.02	.01	.01−	.00+
4		.47	.36	$\underline{.25}$	$\underline{.17}$	.11	.07	.05	.03	.02
5			.48	.41	.33	$\underline{.25}$	$\underline{.18}$	.13	.09	.06
6				.49	.45	.35	.32	$\underline{.25}$	$\underline{.19}$	.14
7					.50−	.47	.43	.37	.31	$\underline{.25}$
8						.50−	.48	.46	.41	.36
9							.50−	.49	.47	.44
10								.50−	.50−	.49
11									.50−	.50−
12										.50−

* To associate values of E_A with values of k for a committee of size n, simply read down the vertical columns of the table. The table is asymmetrical since intersects below the diagonal are impossible ($k > n$). Rule of consensus values appear just on the diagonal ($k = n$). Rule of individual initiative values are in the top row ($k = 1$), and majority rule values ($k > n/2$) are underlined.

We may accomplish this by saying "If Ego belongs to a set of size f, there will remain $n-1$ members to be combined in a set of size $f-1$ in order to bring our set to size f with Ego included." Accordingly, we subtract 1 from n and f in the combinatory formula, obtaining:

$$\frac{(n-1)!}{(f-1)![(n-1)-(f-1)]!}$$

This may be simplified by letting the -1 terms in the lower right hand bracket cancel each other:

$$\frac{(n-1)!}{(f-1)!(n-f)!}$$

This tells us how many support sets of size f will contain Ego. Now, we also need to know the sum of these values for all instances in which the set's size (f) is less than k. This is accomplished by letting the values be summed from $f = 1$ to $f = k - 1$. This accounts for summation term:

$$\sum_{f=1}^{k-1}$$

And the result is the formula given:

$$N_A = \sum_{f=1}^{k-1} \frac{(n-1)!}{(f-1)!(n-f)!}$$

will postpone our analysis of these expectations until they can be combined with those for B (E_B).

(B) Event B occurs whenever our generalized member (Ego) opposes a proposal (is *not* a member of its support set) and the proposal is nonetheless imposed (the set contains at least k members). We may now identify the set of sets which corresponds to event B:

Let: B represent the set of support sets which defines the occurrence of event B

Other notation remains as above.

Then:

$$B \approx [S_f, \text{ such that } f \geq k \text{ and Ego} \notin S]$$

This says simply that set of sets B contains all those support sets which are large enough to impose a policy under decision-rule k and which do not contain our generalized member (Ego). In short, Ego fails to block a policy proposal which his value schedule leads him to oppose.

Under the rule of consensus ($k = n$), set B must be empty, since no support set can equal or exceed k unless it contains our generalized member:

If $k = n$, B empty

For values of k below n, however, a somewhat more complex "counting" problem arises. It is resolved by the formula:[22]

Let: N_B represent the number of support sets in B.

Other notation remains as above.

Then:

$$N_B = \sum_{f=k}^{n-1} \frac{(n-1)!}{f![(n-1)-f]!}$$

$$(\text{If } f = n, N_B = 0)$$

This formula "counts" the number of support sets which are at least large enough to impose a policy under any given decision-rule value (k) and which do not include Ego. This tells how many support sets correspond to event B, in which Ego's value schedule leads him to oppose a policy, yet it is imposed. If we divide this by the total number of support sets in a committee of n members, we may generalize it to the expected frequency for the occurrence of event B:

Expectation for Event B: Any member of a political committee of size n, operating under decision rule k, must expect that a proposal which he opposes will be imposed in:

$$\frac{N_B}{2^n - 1}$$

proportion of the instances in which a proposal comes to a vote. The full formula may be stated:

Let: E_B represent the expected frequency of event B.

Other notation as above.

$$E_B = \frac{\sum_{f=k}^{n-1} \frac{(n-1)!}{f![(n-1)-f]!}}{2^n - 1}$$

$$[f = n, E_B = 0]$$

Like its counterpart for event A, this statement is fully general. Table 2 gives the expected frequencies for event B (E_B) in committees of three to twelve members and decision-rules (k) of 1 to n.

It may be worth pausing to examine the shapes of the curves for the expected frequencies of events A and B (see Figure 1). Both are *monotonic:* the direction in which expected frequencies for events A and B change with k in a committee of any size does not change. The curves have no "humps" or "dips." Second, the expected frequencies for events A and B are *curvilinear:* the rate at which they change with k varies over the values of k. Notice that the rate of change for each curve is greatest around the middle of the k values (i.e., where $k = n/2$). This means, for example, that the expected frequency of event A increases most

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[22] This formula is also derived from the combination rule. In this instance, we wish to "count" the support sets of size f which do not include Ego and for which f is at least k. We may begin by saying, "If Ego is not to be included in a set of size f, there will be $n-1$ members to be combined in sets of size f." We therefore subtract one from n, and leave f unaltered, obtaining:

$$\frac{(n-1)!}{f![(n-1)-f]!}$$

This gives the number of sets of size f which exclude Ego. We wish to sum the value of this fraction for all values of f at or above the value of k. Hence, the summation term:

$$\sum_{f=k}^{n-1}$$

The upper limit is set at $n-1$ for two reasons: (1) we know that no sets belonging to B may be of size n, and (2) this extreme case can produce a minus factorial term, which is nonsensical. The result is the formula given:

$$N_B = \sum_{f=k}^{n-1} \frac{(n-1)!}{f![(n-1)-f]!}$$

TABLE 2. EXPECTED FREQUENCIES FOR EVENT B (E_B)
(IMPOSITION OF OPPOSED POLICY)

k \ n	3	4	5	6	7	8	9	10	11	12
1	.43	.47	.48	.49	.50−	.50−	.50−	.50−	.50−	.50−
2	.14	.27	.36	.41	.45	.47	.48	.49	.50−	.50−
3	.00	.07	.16	.25	.33	.35	.43	.46	.47	.48
4		.00	.03	.10	.17	.25	.32	.37	.41	.44
5			.00	.02	.06	.11	.18	.25	.31	.36
6				.00	.01	.03	.07	.13	.19	.25
7					.00	.00+	.02	.05	.09	.14
8						.00	.00+	.01	.03	.06
9							.00	.00+	.01	.02
10								.00	.00+	.00+
11									.00	.00+
12										.00

sharply as the decision-rule rises from slightly below $n/2$ to slightly above it, and that the rate of change is slowest near the extreme values of k, 1 and n. Third, the expected frequencies for the two events form curves which are perfect mirror images of each other; as intuition would lead one to expect, the curves are *opposed*. Last, both curves vary between *limits of zero and one-half:* no decision rule (k) makes either event more than an "even bet."[23] And, since no event can be both A and B—the two corresponding sets are disjoint—the summed frequency can never exceed half. Let us turn to the summed expected frequencies and an application of the choice criterion.

Evaluation of Decision-Rules under Constitutional Choice Model. We began by looking for a decision-rule which would optimize the expected correspondence between individual values and collective choices. Under such an optimal rule, a generalized member would expect to have a favored proposal defeated or a disfavored proposal imposed less often than under any other rule. This line of reasoning leads us to the choice criterion:

> One should choose that decision-rule which minimizes the sum of the expected frequencies for (A) in which the committee does not impose a policy which his value schedule leads him to support, and (B) in which the committee imposes a policy which his value schedule leads him to oppose.

Our model generates expected frequencies which confirm our intuitive suspicion that no location of the decision-rule will minimize the expected frequencies for both events A and B taken singly. This is because the two curves are opposed; as the expected frequency for A goes down, the expected frequency for B goes up, and *vice versa.* Indeed, the minimum for B is the maximum for A, and the minimum for A is the maximum for B. Under the rule of consensus ($k = n$), one can be sure that no proposal he dislikes will be imposed ($E_B = 0$), but he must also expect that his own proposals will often be defeated ($E_A = .5$). Under the rule of individual initiative ($k = 1$), our member can be sure that proposals he supports will always pass ($E_A = 0$), but he must also expect that many he opposes will also pass ($E_B = .5$). He cannot avoid being outvoted by choosing an extreme decision-rule.

But, as we said, the object is to minimize the *sum* of the expected frequencies for these two events. The optimal decision-rule must then be selected on the basis of the summed expected frequencies for events A and B. The 'best' rule will correspond to the point at which the sum of these frequencies is minimized for a committee of size n. Table 3 gives the summed frequencies in which we are interested.

The outcome of our search for an optimal decision-rule is apparent in these summed values: *majority rule corresponds uniquely to the minimum summed frequency of events A and B in committees with an odd number of members, and shares the minimum with the value n over 2*

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[23] This upper limit occurs because there is only a .5 probability that Ego will vote yes (or that he will vote no.) These expected frequencies must not, then, be confused with the conditional probability that A or B will occur once Ego has voted yes or no.

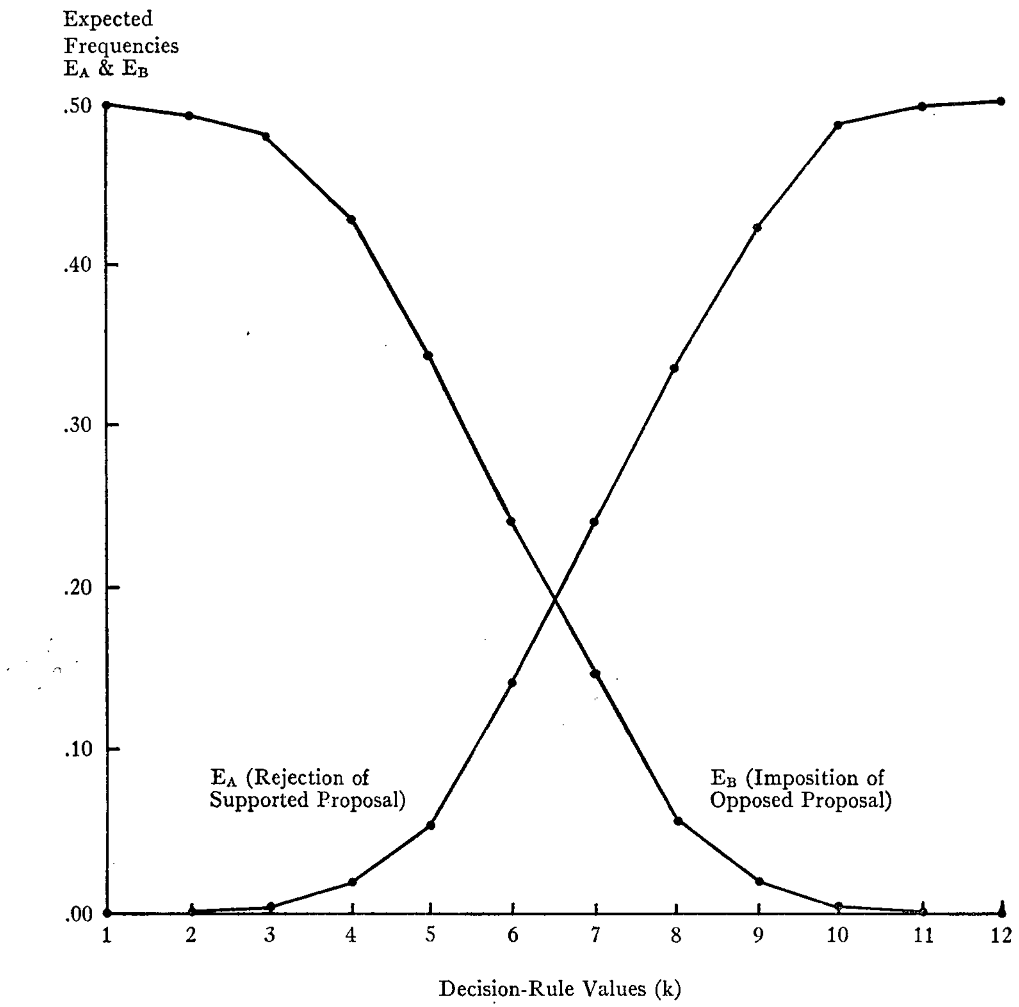

FIG. 1. Expected Frequencies for Events A and B ($n = 12$, $k = 1 \ldots 12$).

NOTE: These expected frequencies apply to long agenda, and may be equated with probabilities for single items on agenda. The reader may use the tables to draw curves for other values of n and k.

TABLE 3. EXPECTED SUMMED FREQUENCIES FOR EVENTS A AND B

k \ n	3	4	5	6	7	8	9	10	11	12
1	.43	.47	.48−	.49−	.50−	.50−	.50−	.50−	.50−	.50−
2	.28	.34	.39	.43	.46	.47+	.48+	.49+	.50−	.50−
3	.43	.34	.32	.35	.39	.38	.45	.47	.48	.48+
4		.47	.39	.35	.34	.36	.39	.42	.44	.46
5			.48	.43	.39	.36	.36	.38	.40	.42
6				.49	.46	.38	.39	.38	.38	.39
7					.50−	.47+	.45	.42	.40	.39
8						.50−	.48+	.47	.44	.42
9							.50−	.49+	.48	.46
10								.50−	.50−	.48+
11									.50−	.50−
12										.50−

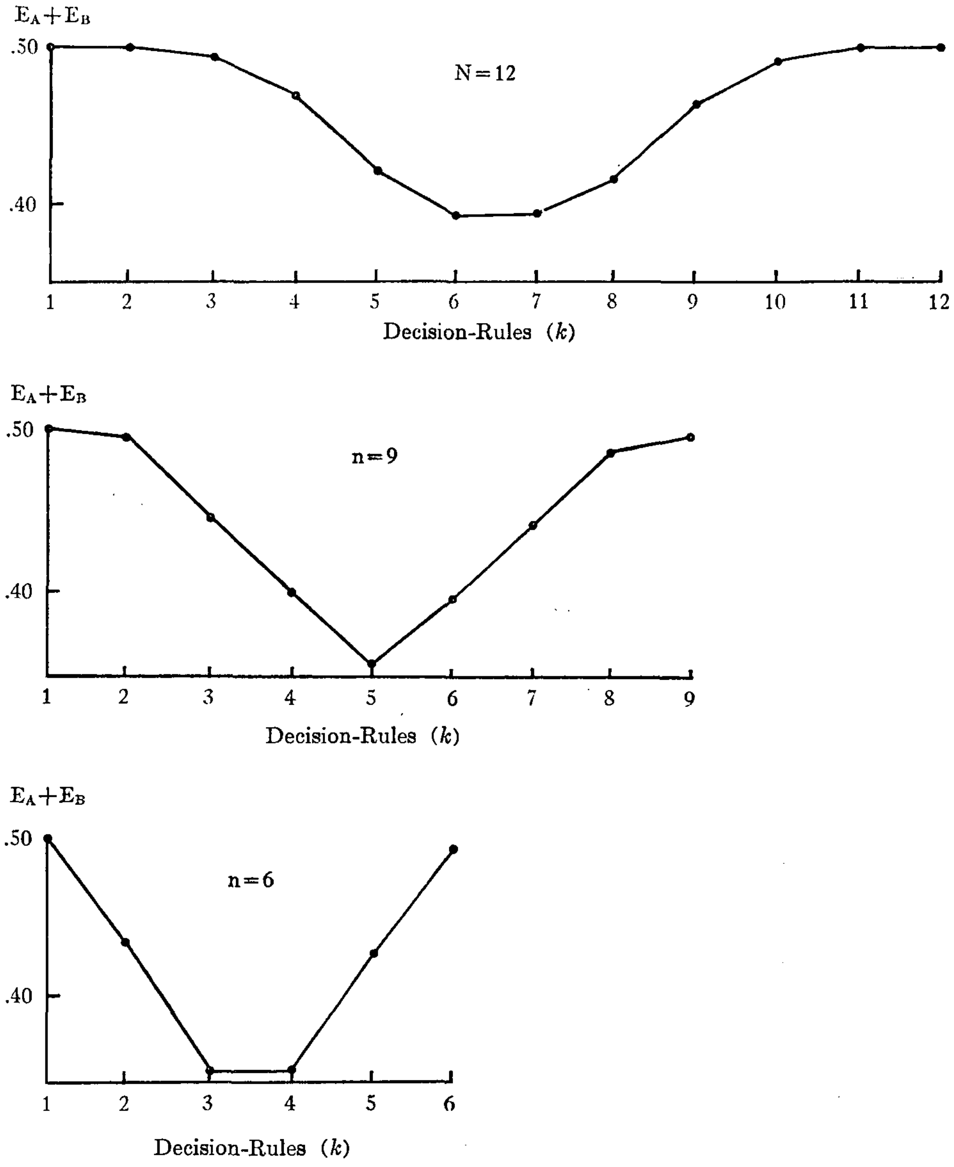

FIG. 2. Expected Summed Frequencies for Events A and B (n as indicated, $k=1\ldots n$).

in committees with an even number of members. If, under our model, one applies the choice criterion to the range of possible decision-rules, he will be compelled to plump for majority-rule or an adjacent value in every instance. As one proceeds toward the rule of consensus or toward the rule of individual initiative, the expected summed frequencies for events A and B increase. These relations may be seen graphically in Figure 2, and our findings may be stated as follows:

Proposition 1: In committees with an odd number of members, the optimal decision-rule is majority-rule: n over two, plus one-half.

$$k\,[\text{optimal}] = \frac{n}{2}+\frac{1}{2}, \text{ or } \frac{n+1}{2}$$

Proposition 2: In committees with an even number of members, the optimal decision-rule is either majority-rule, or majority-rule minus one: n over two plus one, or simply n over 2.

$$k\,[\text{optimal}] = \frac{n}{2}+1, \text{ or } \frac{n}{2}$$

In so far as the model holds, these propositions are general, and our choice criterion requires that we select one of the values specified. If one confronts a choice similar to the one confronted in our model by Ego, and if he accepts the major tenets of the model itself, he will find no k value which improves on majority rule:

Proposition 3: In any political committee, majority-rule is as good (i.e., optimal) as any alternative decision-rule, given the model proposed here.

This assertion is, in a sense, more general than existing arguments for majority-rule, since it rests on a comparison with all of its alternatives. And, since the value premise is only an open preference for an optimal correspondence between individual values and collective choices, whatever the individual values may be, the argument may be somewhat less brittle than those which rest on more dogmatic and overarching premises.

The recommendation is also somewhat more general than our (admittedly restrictive) assumptions suggest. These assumptions are meant to represent the problem of "constitutional choice," not to maximize the generality of our conclusion. *Majority-rule is the one decision-rule which precludes the possibility that more people will be outvoted by less people.*[24] This accounts for the present conclusion, and it drives us to comparable judgement under any set of assumptions, so long as we can speak of a generic individual[25] (as we cannot in the "standing minority" problem considered in Part III below.) This generality is illustrated by the appendix to this paper and is proved by Michael Taylor in a forthcoming article.[26] It may, however, be more interesting to consider two sets of assumptions under which our conclusion does *not* apply.

III. SOME ALTERNATIVES

While this recommendation has a certain power of its own, we must remember that "models are models," and that we have quite consciously put aside important aspects of experience. The pure case of constitutional choice is probably a very rare empirical event, for the choosers of a decision-rule are apt to at least think they know enough about the future to make more determinate guesses than the ones we have made in our model. Let us examine some of the implications which follow from added knowledge about the future.

Let us suppose, first, that a member entertains a *positional* (as opposed to substantive) *preference,* which leads him to think bad actions (Event B) are worse than bad inactions (Event A). This might be so, for example, if he were a "conservative" in Samuel Huntington's sense of that term;[27] or if there were some *a priori* difference in the consequences of action and inaction, as with criminal juries in which Ego might consider a bad conviction worse than an objectionable acquittal. And, second, let us suppose that we may anticipate the formation of *factions* whose members possess identical preferences over some class of issues. If this is so, we must suppose that a "permanent minority" may arise and be exploited by a permanent (or rotating) majority. In the following paragraphs, we consider the effects of these guesses about the future on our recommendation for majority-rule.

Positional Preferences. Let us suppose that a member considers it worse for the committee to act against his wishes (Event B) than for it to refuse action in accordance with his wishes (Event A). It appears, for example, that a majority of delegates (including the articulate Madison) at the Philadelphia convention thought it would be worse if Congress expelled a member over a substantial number of objections than if it failed to do so under comparable circumstances.[28] Beliefs of this kind—usually related to specific classes of proposals—lead us to assign different "weights" to Events A and B. We may begin thinking about the consequences of these beliefs by defining the admittedly arbitrary quantities which correspond to these weights:

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[24] This is because all more restrictive rules allow negative minorities to outvote positive majorities, while less restrictive rules allow positive minorities to outvote negative majorities. Under all such rules, it is therefore possible that Event A or Event B will occur for more than half the members on a single vote: under majority-rule (and under $k = n/2$ in committees of even size) this is not possible.

[25] And so long as we do not consider "positional preferences," as discussed in Part III below.

[26] "A Proof for Rae's Majority-Rule Theorem," forthcoming, *Behavioral Science* (Summer, 1969).

[27] In "Conservatism as an Ideology," this REVIEW, Vol. 51, Huntington describes conservatism as a "positional ideology," leading its proponent to an ". . . articulate, systematic, theoretical resistence to change." (p. 461.) This suggests that a conservative would be apt to weight Event B (unwanted change) more heavily than Event A (unwanted stability).

[28] This is an extrapolation from James Madison's *Journal of the Federal Convention,* Vol. 2 (Chicago: Albert Scott and Co., 1893) p. 500. Since the premises of the argument are inexplicit, one cannot be sure that any interpretation is historically correct; the interpretation offered here is, however, at least as plausible as any alternative.

1. Let α represent the weight attached by an individual to Event A, failure to impose a proposal he likes.
2. Let β represent the weight attached by an individual to Event B, adoption of a proposal he dislikes.

We must note that these weights are conceptually distinct from the individual's preference on a given proposal. They rest instead on the assertion that, for all proposals within some class, an A outcome may be assigned a weight distinct from that assigned to a B outcome. Given these weights, our criterion of choice is no longer dependent upon the simple sum of the expected frequencies for Events A and B, but on the weighted sum of these frequencies. This revised value assumption recommends the decision-rule which minimizes the value of the weighted frequency:

The optimal rule minimizes: $\alpha \cdot E_A + \beta \cdot E_B$

Drawing the expected frequencies from our simple model (or some other model) and the weights by "fiat" from our hypothetical individual, we may now reevaluate a given set of possible decision-rules. Putting aside the enormous difficulty of supplying meaningful quantities for α and β, let us illustrate this alteration of our calculations for a seven-member committee. In this example, we assign weights of one and two to α and β. The question then becomes, "Do these weights justify the adoption of a decision-rule more restrictive than the majority?" On the assumption that the weights themselves make sense, we may answer this question with the weighted frequency term set forth above. In our example, we may answer this question by calculating this term for $n = 7$, $k = 1 \ldots 7$, $\alpha = 1$, and $\beta = 2$. The results are shown in the right-hand column of Table 4.

We see that a two-thirds (i.e., $k = 5$) rule is to be preferred in this instance over the simple majority (i.e., $k = 4$) and over all other alternatives. This illustrates the plausibility of restrictive rules where we can say that bad action is worse than bad inaction, and the "calculations" may very well be analogous to those which underlie the argument for such rules in, say, criminal juries or in bodies which decide upon proposals for constitutional revision. This is not to say that actors have literally made calculations like those given here, but rather that they may have thought the question out in less formal terms and relations similar to those considered here.

TABLE IV. "WEIGHTED" FREQUENCY CALCULATION ($n = 7$, $k = 1 \ldots 7$, $\alpha = 1$, $\beta = 2$)

k	α	E_A	$\alpha \cdot E_A$	β	E_B	$\beta \cdot E_B$	$\alpha \cdot E_A + \beta \cdot E_B$
1	1	.00	.00	2	.50	1.00	1.00
2	1	.01	.01	2	.45	.90	.91
3	1	.06	.06	2	.33	.66	.72
4	1	.17	.17	2	.17	.34	.51
5	1	.33	.33	2	.06	.12	.45
6	1	.45	.45	2	.01	.02	.47
7	1	.50	.50	2	.00	.00	.50

This brings us to the suggestion that the simple model presented in Part II is a special case of this more general model: it is the case in which α equals β. In a formal sense, this is self-evident, but we should remember that it is an especially interesting case, for the assumption of equal weights is in general more plausible than any alternative. This is especially evident for political committees whose agenda are open to all types of proposals and which confront dynamic environments which must be expected to undermine any special argument for the *status quo*. In the absence of special evidence to the contrary—like that cited for juries—it seems prudent to accept the equal-weighting case.

Factions. Let us put the problem of factions as bluntly as possible.[29] Suppose that we know enough about the committee and its long-term agenda to say that there will, for some class of proposals, be two factions, one of which will contain a majority and one of which will contain a minority of the members. And, suppose further, that we can expect a conflict of interest[30] between these factions for some class of issues, so that their preferences will be opposed on each proposal within that class. If we assume all this—as we might in some important cases—then we confront a bald case of the classic difficulty in majoritarian democracy: the exploitation of standing minorities by standing majorities. In our model, Assumption I sheltered us from this prospect, and we may now consider its implications briefly. To begin with, say that:

1. A majority faction contains q members ($q > n/2$).
2. A minority faction contains $n - q$ members ($< n/2$).

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[29] The following example states the problem in the simplest possible way, and it is just one of many sorts of examples which might be devised. If one considers more than two factions, and partial degrees of conflict, the argument is vastly more complex, but the essential conclusion of this section is probably unaffected: the prospect of factional exploitation is not a unique attribute of majority-rule.

[30] One useful analysis of this concept is offered by Robert Axelrod, *Conflict of Interest: Theory and Political Applications* (unpublished Ph.D. thesis, Yale University, 1968).

3. These two groups have opposed preferences for all issues within the class to be considered.

If we consider a "random" individual, one with q/n chances of belonging to the standing majority, it is obvious that majority rule is at least as desirable as any alternative. This, however, avoids the problem, so we must consider two discrete individuals, one a member of the standing majority and one a member of the standing minority. As Figure 3 suggests, there are only three classes of decision-rules which need concern either of these individuals: (1) those which require no more votes than the number commanded by the minority $[k \leq (n-q)]$; (2) those which require more votes than the number commanded by the minority but no more than are commanded by the majority $[k > (n-q),\ k \leq q]$; and (3) those which require more votes than are commanded by the majority $(k > q)$. Given our assumption that the members of these two factions disagree for all proposals within the class considered (3, above), we have only these three kinds of rules to consider.

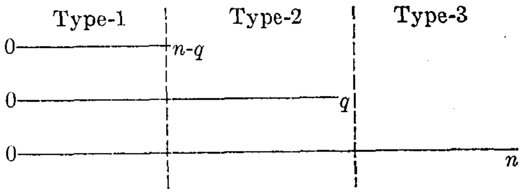

FIG. 3. Three classes of decision-rules for a committee with exactly two consistently opposed factions.

For a member of the standing majority, it is obvious that Type-2 decision-rules are preferable to types 1 and 3. Under Type-2 rules—majority-rule is an example—this member will expect to have his way on all issues belonging to the class considered here. This is because the majority faction can impose all proposals its members like $(E_A = 0)$, and veto all proposals its members dislike $(E_B = 0)$. Under Type-3 rules, however, he must expect not to have his way on proposals which he and his majority fellows like, for the opposed minority will surely veto them (E_A = the proportion of proposals he likes). And under Type-1 rules, he must expect not to have his way on proposals he dislikes, since the minority will certainly impose them (E_B = the proportion of proposals he dislikes). We would therefore expect the majority member to prefer decision-rules of Type-2—majority-rule, for example.

In contrast, we must expect a minority member to prefer either Type-1 or Type-3 rules, and not Type-2 rules. This is obvious, since Type-1 allows him to impose policies he likes $(E_A = 0)$, and Type-3 rules allow him to veto policies he dislikes $(E_B = 0)$, while Type-2 rules allow him neither of these opportunities. And, given our assumptions, there is no possibility of compromise—save the adoption of different rules for various sub-classes of issues.

This bald example of the problem raised by standing majorities and minorities leads to the conjecture that *the problem of the exploited faction is not a special difficulty of majority-rule, but a generic difficulty of all simple-decision-rules*. This is so because *any* rule will leave either the minority or the majority subject to one of three unhappy prospects: the unchecked imposition of unwanted policies; or the invariable veto of wanted policies; or both. It may, of course, be said that Type-1 and Type-3 rules are less open licenses to exploitation than Type-2 rules, since they at least open the prospect of strategic bargaining by the minority. Yet it is hard to imagine the majority which would willingly accept such rules.

Even this much-too-brief consideration of factional conflict suggests that the effort to optimize individual values in collective choice must, in such instances, look beyond the choice of simple decision-rules. No such rule will produce a joint optimization (as did majority-rule under the simple model). Separatism—be it complete, as with the Pakistani withdrawal from India, or partial, as in federal and confederate constitutions—may well be the only prospect for a joint optimization through constitutional choice. Still, separation itself raises the serious prospect that neither faction will, in the long run, fare very well.[31] Admitting that this consideration of the problem is far from exhaustive, I suspect that one must look away from formal institutional choices and toward the standard civic virtues if individual values are to be optimized in factional strife.

IV. CONCLUSIONS

We arrive at three main conclusions. First, majority rule can be expected to optimize the correspondence between individual values and collective choice under the assumptions of our simple model. Only under a model of this kind can we expect unanimous *a priori* agreement on decision-rules, for the introduction of other information produces determinate conflicts of interests—as illustrated by our second and third conclusions. The second conclusion is that positional preferences constitute a logically legitimate basis for the choice of non-majoritarian decision-rules. It follows that, if there is a conflict over positional preferences, there must also be a conflict over decision-rules, even if other assumptions are unaltered. Third, if we consider the "bald case of factional conflict," it appears that the problem of the exploited faction is general to all simple decision-rules, and not a special property of majority rule. In this case, we cannot expect factions to agree on any given decision-rule.

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[31] In the terms of this analysis, we might say that partition introduces a special class of events in which policies which Ego favors, and which would have been adopted before the partition, are rejected because the required resources are no longer available. These events are analogous to Event A—the rejection of proposals which Ego favors. These events are singularly relevant to proposals for separation of minorities, as with French Quebec or Black America, since the minority communities would operate with vastly restricted resources.

These conclusions lead to one last conjecture: a constitutional choice can be made consensually only if uncertainty about the future makes appropriate a model analogous to our simple one. Once decision-rules are conceived as instruments in a determinate conflict of interests, then the incentive for agreement on majority-rule (or any alternative to it) is destroyed, and unanimous consent seems improbable. There may be many ways to obtain agreement on rules in the face of disagreement on issues, but a self-conscious ignorance of the future is probably not the least important among them.

APPENDIX

A mathematical proof devised by Michael Taylor demonstrates that the outcome of the simple models—the optimality of majority-rule—depends only on Assumption I and not on Assumptions II and III.[32] This means that the recommendation is general to *all* probabilities for yes votes, regardless of whether or not we take "non-proposals" opposed by all members into account. In effect, Taylor's proof suggests that Assumptions II and III are theoretically trivial, having simplicity of exposition as their sole purpose. This may be illustrated here by giving a few computational examples which violate Assumptions II and III but nevertheless lead to confirmations of Propositions 1, 2, and 3 (the optimality of majority-rule). In each, we disregard Assumption III and give a specific alternative to Assumption II.

example one:

Consider the following alternative to Assumption II:

$$P_X = \frac{1}{3}, \; P_x = \frac{2}{3}.$$

Under our new assumptions, there are eight possible support sets in a committee of three (one with three members, three with two, three with one, and one empty). Using the probabilities assigned under this alternative we calculate:

$$P_{XYZ} = \frac{1}{3} \times \frac{1}{3} \times \frac{1}{3} = \frac{1}{27}$$

$$P_{YZ} = P_{XZ} = P_{XY} = \frac{1}{3} \times \frac{1}{3} \times \frac{2}{3} = \frac{2}{27}$$

$$P_X = P_Y = P_Z = \frac{1}{3} \times \frac{2}{3} \times \frac{2}{3} = \frac{4}{27}$$

$$P_{empty} = \frac{2}{3} \times \frac{2}{3} \times \frac{2}{3} = \frac{8}{27}$$

$$\left[\frac{1}{27} + \frac{(3 \times 2)}{2} + \frac{(3 \times 4)}{27} + \frac{8}{27} = 1\right]$$

As before, we may define Events A and B as sets of support sets, and, letting X (or Y or Z) represent Ego, we may calculate the expected frequencies for these events:

Decision-Rule (k)	*Expected Frequency Event A*	*Expected Frequency Event B*	*Sum of A & B*
1	0	$\frac{10}{27}$	$\frac{10}{27}$
2	$\frac{4}{27}$	$\frac{2}{27}$	$\frac{6}{27}$
3	$\frac{8}{27}$	0	$\frac{8}{27}$

Notice that the summary frequency for the two events is no longer symmetrical, but, more important, notice that the minimum for that curve remains at $k=2$, and our finding is unaltered. This is not changed if we reverse the probability assumption, saying that members are twice as apt to support as to oppose each proposal; the curves rotate 180°, but the minimum has the same value and the same location. Or, if we return to Assumption III, barring empty support sets from occurrence, the devisor in each fraction becomes 19 instead of 27, but the location of the A+B minimum is unaltered.

example two:

Now consider an extreme case, in which each of three members is nine times as apt to support as to oppose any proposal:

$$P_X = \frac{9}{10}, \; P_x = \frac{1}{10}.$$

This activist committee produces support sets with the following probabilities:

$$P_{XYZ} = \frac{9}{10} \times \frac{9}{10} \times \frac{9}{10} = \frac{729}{1000}$$

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[32] Taylor, *op. cit.*

$$P_{XY} = P_{XZ} = P_{YZ} = \frac{9}{10} \times \frac{9}{10} \times \frac{1}{10} = \frac{81}{1000}$$

$$P_X = P_Y = P_Z = \frac{9}{10} \times \frac{1}{10} \times \frac{1}{10} = \frac{9}{1000}$$

$$P_{empty} = \frac{1}{10} \times \frac{1}{10} \times \frac{1}{10} = \frac{1}{1000}$$

$$\left[\frac{729}{1000} + \frac{(3 \times 81)}{1000} + \frac{(3 \times 9)}{1000} + \frac{1}{1000} = 1\right]$$

Expected Frequencies for Events A and B may be computed from these probabilities under our set definitions, and the result is:

Decision-Rule (k)	*Expected Frequency Event A*	*Expected Frequency Event B*	*Sum of A & B*
1	0	$\frac{99}{1000}$	$\frac{99}{1000}$
2	$\frac{9}{1000}$	$\frac{81}{1000}$	$\frac{90}{1000}$
3	$\frac{171}{1000}$	0	$\frac{171}{1000}$

The minimum expected frequency for Events A and B still falls at $k=2$. Admittedly, under these extreme assumptions, it is only slightly lower than the value for $k=1$, but its location is nevertheless stationary. Notice too, that the range of values for this summed frequency is quite low for all three values of k: our activist assumption has placed most of the members on the winning side of most issues (i.e., in 730 of 1000 instances, all three would vote together). Turning this activist committee into a meeting of moss-backs (i.e., reversing the assumption) simply reverses the curves, leaving their shapes and the location of the A+B minimum where it was under Assumption II. And returning to Assumption III simply reduces the denominator in each fraction to 999 instead of 1000.

example three:

As a last example, let us consider an absurd extreme, in which members support proposals with a probability of .999, and oppose them with a probability of .001. Here, we obtain:

$$P_{XYZ} = \frac{997{,}002{,}999}{1{,}000{,}000{,}000}$$

$$P_{XY} = P_{XZ} = P_{YZ} = \frac{998{,}001}{1{,}000{,}000{,}000}$$

$$P_X = P_Y = P_Z = \frac{999}{1{,}000{,}000{,}000}$$

$$P_{empty} = \frac{1}{1{,}000{,}000{,}000}$$

$$\left[\frac{997{,}002{,}999}{1{,}000{,}000{,}000} + \frac{(3 \times 998{,}001)}{1{,}000{,}000{,}000} + \frac{(3 \times 999)}{1{,}000{,}000{,}000} + \frac{1}{1{,}000{,}000{,}000} = 1\right]$$

Decision-Rule (k)	*Expected Frequency Event A*	*Expected Frequency Event B*	*Sum of A & B*
1	0	$\frac{999{,}999}{1{,}000{,}000{,}000}$	$\frac{999{,}999}{1{,}000{,}000{,}000}$
2	$\frac{999}{1{,}000{,}000{,}000}$	$\frac{998{,}001}{1{,}000{,}000{,}000}$	$\frac{999{,}000}{1{,}000{,}000{,}000}$
3	$\frac{1{,}997{,}001}{1{,}000{,}000{,}000}$	0	$\frac{1{,}997{,}001}{1{,}000{,}000{,}000}$

It should be clear from these examples that the optimum is stable, at least for very small committees. Taylor's forthcoming proof generalizes this stability to all alternatives for Assumptions II and III, for committees of all sizes. It is quite clear that the degree to which majoritarian decision-rules are superior to their alternatives declines with size[33] and with the asymmetry of the probability assumption, but the nominal fact of their optimality remains in all cases.

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[33] This suggests an incentive for the formation of minimum winning coalitions, even if we assume that preferences are independently arrived at so that there is no *a priori* basis for group cohesion. This is because the expected frequency for events A and B is lower, for any given k, in small committees than in large committees. By forming a coalition of k members, and operating under the analogous decision-rule internally, members can always have their way slightly more often than they would by voting independently. In a committee of five, with independent voting, members can expect to have their way 11/16ths of the time if $k=3$; by forming a coalition of three, however, it is possible to raise this expectation to 12/16ths, if $k=2$ is adopted by the coalition. This suggestion is not taken into account by our simple model, which is based on sincere individual voting. This suggestion is an interesting parallel to William Riker's prediction of minimum winning coalitions on the basis of a quite different model. See *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962.)

THE PARTY VARIABLE IN JUDGES' VOTING: CONCEPTUAL NOTES AND A CASE STUDY*

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I. INTRODUCTION

Since the writings of the judicial realists in the 1920's and 1930's almost all social scientists have accepted the concept that the judicial process is basically political. One line of reasoning points out that judges inevitably are policy makers because of their functions, such as constitutional and statutory interpretation. Politics is conflict among interests or values or demands, and any activity, including judicial decisions, which advantages one interest as opposed to others is political activity. Since judges decide cases involving interest conflicts, theirs is "interest activity not as a matter of choice but of function."[1] Robert H. Jackson made the point cogently when he argued that the "Supreme Court has, from the very nature of its functions, been deep in power politics . . ."[2] Of the power of judicial review, he said, "The ultimate function of the Supreme Court is nothing less than the arbitration between fundamental and ever-present rival forces or trends in our organized society."[3] Although these remarks were addressed to the Supreme Court, they are true to a substantial degree of judicial power throughout the American system. Many of the conflicts adjudicated by the judiciary advance some interests or values and disadvantage others.

At the same time that the judicial function has been recognized as political in its consequences, the process of judicial decision making has come to be viewed as involving wide opportunities for discretion by the judges. The mechanical theory of judging, enunciated most clearly by the late Justice Owen Roberts,[4] is now generally in disrepute, and realists prefer to cite Charles Evans Hughes who stated "We are under the Constitution, but the Constitution is what the judges say it is."

This view of the judicial process has led in recent years to an exhaustive search for determinants of judges' conduct. The importance of the value preferences of the justices is commonly noted. Judicial conduct may be affected by forces such as the litigants, interest groups which sponsor test cases and file amicus curiae briefs,[5] law professors, law reviews, the bar associations, community groups which bestow honors and recognitions,[6] and even law clerks, although their influence is hotly denied.[7] The judicial role as judges perceive it may modify and guide their conduct. The selection process may also shape judicial decision making by installing in judicial office men of certain kinds of philosophic and policy outlooks.[8] Certainly the thrust of Robert Dahl's analysis of the Supreme Court

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* I am grateful for the comments and advice of Fred I. Greenstein, Clement E. Vose, Joel B. Grossman and Kenneth Dolbeare on early drafts of this paper. I wish to thank Gaylon Greenhill, chairman of the Department of Political Science at Wisconsin State University—Whitewater, and Marvin Summers and Wilder Crane, chairmen of the Department of Political Science at the University of Wisconsin—Milwaukee, for the considerable assistance rendered me at those institutions during my appointments there.

[1] Jack Peltason, *Federal Courts in the Political Process* (New York: Random House, 1955), ch. 1, esp. p. 3.

[2] Robert H. Jackson, *The Struggle for Judicial Supremacy* (New York: Random House-Vintage, 1941), p. vii.

[3] *Ibid.*, p. 311.

[4] *United States v. Butler,* 297 U.S. 1, 79 (1936).

[5] Clement E. Vose, "Litigation as a Form of Pressure Group Activity," *The Annals of the American Academy of Political and Social Science,* 319 (September, 1958), 20–31; and his "Interest Groups, Judicial Review, and Local Government," *Western Political Quarterly,* 19 (March, 1966), 85–100. The Court's reaction in 1949 to the excessive use of amicus briefs is discussed in Fowler V. Harper and Edwin D. Etherington, "Lobbyists Before the Court," *University of Pennsylvania Law Review,* 11 (1953), 1172–1179. See also Samuel Krislov, "The Amicus Curiae Brief: From Friendship to Advocacy," *Yale Law Journal,* 72 (1963), 694–721.

[6] On these points, see Jack Peltason, *op. cit.,* pp. 22–25, for a review of some of the sources and findings.

[7] Various views on the influence of law clerks in the decisional process of the United States Supreme Court are presented in Glendon Schubert, *Constitutional Politics* (New York: Holt, Rinehart and Winston, 1964), pp. 138–146.

[8] John Schmidhauser, *The Supreme Court: Its Politics, Personalities and Procedures* (New York: Holt, Rinehart and Winston, 1963), chs. 2–3.

as a national policy-maker is that eventually, through the process of appointment by the President and confirmation by the Senate, the Court is brought into line on policy matters with the long-term law-making majority representing the fairly stable popular majority coalition in the country.[9]

There has developed, in this search for determinants of judicial conduct, a growing and oft-cited body of literature linking the party affiliations of judges to their votes in cases before them.[10] This literature has invited an increasing number of generalized statements that party is a significant variable in judicial behavior. It is the purpose of this paper to comment briefly on the various conceptualizations of partisanship as a determinant of judicial behavior, to replicate with Wisconsin data the studies which seem conceptually sound, and to suggest additional factors which might modify the tendency to generalize the hypothesis that party identification per se is an influence on judges' behavior.

II. THE PARTISAN VARIABLE: REVIEW AND COMMENT

Glendon Schubert found through simple roll call analysis a sharp cleavage along party lines on the Michigan Supreme Court on the issue of workmen's compensation claims, with Democratic justices far more friendly to workers' claims than their Republican brethren.[11] For the 1954–1957 Terms of the Court, he also found clear evidence of cohesive dissenting blocs drawn along party lines.[12] In a subsequent study relying primarily on similar roll call analysis and on modified scalogram techniques, S. Sidney Ulmer affirmed a sharp division of Michigan Supreme Court justices along party lines on the issues of workmen's and unemployment compensation for the 1958–1960 Terms of that Court.[13] He concluded that "Democratic justice is more sensitive to the claims of the unemployed and the injured than Republican justice."[14]

Stuart S. Nagel studied the party affiliations and voting records of state and federal high court judges in fifteen areas of public and private law for the year 1955.[15] He computed a "decision score" for each judge on those high courts whose members were divided, by previous affiliation, between adherents of the two major parties. The "decision score" was the percentage of votes in each area cast for the "liberal position" which he defined as "the viewpoint associated with the interests of the lower or less privileged economic or social groups in one's society and (to a less extent) with acceptance of long-run social change . . ."[16] Thus, for instance, votes for the defense in criminal cases, the administrative agency in business regulation cases, the claimant in unemployment cases, the wife in divorce settlement cases, and the employee in employee injury cases were regarded as "liberal" votes. On this basis, Nagel found that, to a statistically significant degree, Democratic judges cast more votes for the "liberal" position than their Republican colleagues in nine of the fifteen areas of law analyzed.

The correlation between prior party affiliation and judicial voting was found to be less significant in states with appointive high courts than in states in which high court judges were elected. Among the elected judges, however, the length of term (shorter or longer than eight years) and the format of the ballot (partisan or nonpartisan) did not affect the relationship between party affiliation and judges' policy preferences.[17]

Sheldon Goldman[18] used techniques similar to Nagel's. His study of voting by U.S. Court of Appeals judges from 1961 to 1964 led him to conclude that Democrats on these courts "appear relatively more 'liberal' than Republicans in cases which involve what might be called 'economic liberalism.' "[19] He did not find a relationship between party affiliation and judges' voting in civil liberties or criminal cases.[20]

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[9] "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker," *Journal of Public Law,* 6 (1957), 279–295.

[10] Glendon Schubert, *Quantitative Analysis of Judicial Behavior* (Glencoe, Illinois: The Free Press, and East Lansing, Michigan: Michigan State University Bureau of Social and Political Research, 1959), pp. 129–142, esp. pp. 132–134. S. Sidney Ulmer, "The Political Party Variable on the Michigan Supreme Court," *Journal of Public Law,* 11 (1962), 352–362. Stuart S. Nagel, "Political Party Affiliation and Judges' Decisions," this REVIEW, 55 (December, 1961), 843–850. Sheldon Goldman, "Voting Behavior on the United States Courts of Appeals, 1961–1964," this REVIEW, 60 (June, 1966), 374–383. A study of both regional and party background variables at a much earlier period in American history is John Schmidhauser's "Judicial Behavior and the Sectional Crisis of 1837–1860," *Journal of Politics,* 23 (November, 1961), 615–640.

[11] Schubert, *op. cit.,* pp. 132–133.

[12] *Ibid.,* pp. 135–142.

[13] Ulmer, *op. cit.,* pp. 352–362.

[14] *Ibid.,* p. 362.

[15] Nagel, *op. cit.,* pp. 843–850.

[16] *Ibid.,* p. 846.

[17] *Ibid.,* pp. 848–849.

[18] *Op. cit.*

[19] *Ibid.,* p. 381.

These studies raise a major conceptual question: what is the link between party and judicial conduct? Most of the studies show nothing more than that certain decisional propensities are associated with the characteristic of prior party affiliation. While it is useful to know of this association, both Schubert[21] and Grossman[22] have recently warned that background characteristics are an inadequate explanatory device because they ignore the existence of a range of other factors which operate upon the judge.

Political scientists have developed no evidence that party operates in the judicial process through the formal mechanisms common in other branches of government—through legislative caucuses or the leadership of executives, for instance. The link between party and decision-making need not rest on such devices, however. Party identification and the socializing process of party activism may impart issue orientations which affect the decisions made by officeholders, including judges.

Nagel has suggested that party affiliation and judges' votes may "correlate with each other because they are frequently effects of the same causes," and he argues that the common causes are the "personal standards of value" held by judges.[23] This view ignores the role that party identification and party activism play in developing issue orientations. Although recognizing that the recruitment process may tend to bring into a party those who agree with its principles or values, Fred I. Greenstein and Elton F. Jackson argue that party affiliation may shape values and thus influence voting in the legislature.

> . . . party affiliations often are acquired early in the individual's life, prior to the formation of attitudes on issues; they are extremely stable; and they evidently are a major influence on issue and candidate evaluations. But when voters are asked to explain their electoral choices, they normally refer to issues and candidates rather than party. It is likely that party identification has similar effects on legislative voting behavior.[24]

Their comments may be pertinent to judicial behavior as well.

If judges are party identifiers before reaching the bench, there would be a basis for believing that they—like legislators—are affected in their issue orientations by party. Nagel has shown that 271 of the 304 justices of state high courts in 1955 had clear prior party affiliations.[25] Furthermore, judges are generally well educated and the vote studies show that the more educated tend to be stronger party identifiers, to cast policy preferences in ideological terms, to have clearer perceptions of issues and of party positions on those issues, to have issue attitudes consistent with the positions of the party with which they identify, and to be more interested and involved in politics. For judges, even more than for the general population, party may therefore be a significant reference group on issues.

Party activism is an even greater force than party identification in shaping policy orientations, and the available evidence indicates that judges have usually been party activists before taking the robe. Schmidhauser shows that United States Supreme Court justices have, with only a single exception, held some political post before going to the bench and that most of them were active in highly partisan forums, such as the state and national legislative and executive branches.[26] The study by Mott *et. al.*, which is now regrettably out of date, shows that in a thirty year period most judges of the highest courts in thirty-two states had prior experience in public legal offices, public administrative offices or legislative offices, most of them of a kind filled by appointment or election on a partisan basis.[27]

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[20] *Ibid.* He hedges this finding somewhat by pointing out that his data permit an interpretation which suggests that "on the really 'important' cases that force the judges to stand up and be counted," the Democrats have higher overall liberalism scores and that these might include a higher degree of liberalism on some civil liberties and criminal matters.

[21] Glendon Schubert, *Judicial Policy-Making* (Chicago: Scott, Foresman and Co., 1965), p. 113.

[22] Joel B. Grossman, "Social Backgrounds and Judicial Decisions," *Journal of Politics,* 29 (May, 1967), 334–351, esp. pp. 344–347.

[23] Nagel, *op. cit.,* p. 847.

[24] Fred I. Greenstein and Elton F. Jackson, "A Second Look at the Validity of Roll-Call Analysis," *Midwest Journal of Political Science,* 7 (May, 1963), p. 161.

[25] Stuart S. Nagel, "Unequal Party Representation on State Supreme Courts," *Journal of the American Judicature Society,* 45 (1961), 62–65.

[26] John Schmidhauser, "The Justice of the Supreme Court—A Collective Portrait," *Midwest Journal of Political Science,* 3 (February, 1959), p. 37.

[27] Rodney Mott, Spencer D. Albright and Helen Semmerling, "Judicial Personnel," in Glendon Schubert (ed.), *Judicial Behavior: A Reader in*

McClosky has shown that on public issues political activists divide more sharply along party lines than do the voters as a whole, and he argues that party plays an important socializing role which affects the policy orientations of activists.[28] Party activism leads to a greater party spirit, to a greater sense of belonging to and identifying with a party, and thus very often establishes a motivation to accept the doctrines common in the party and to learn how those doctrines differ from the doctrines of the opposition party.[29] Furthermore, "since politics is more salient for leaders than for followers, they develop a greater stake in the outcome of the political contest and are more eager to discover the intellectual grounds by which they hope to make victory possible."[30] Also, a belief in party doctrine may serve as a self-justification for political activity; it casts the commitment of time, money, and energy in terms of serving significant social goals.[31] Finally, the number of political activists is small and their interpersonal contacts are almost entirely with those who agree with the party's doctrines and do so articulately, thus developing or greatly re-enforcing commitment to party policy orientations within the group.

It seems likely that party identification is one of the psychological dispositions through which judges view issues before they go to the bench. This disposition is strengthened by the socializing process of their situational circumstances, especially their party activity. The query remains whether other psychological dispositions and socializing processes become important in the new situational circumstance, that of being a judge, which significantly diminish the importance of party in making policy choices.

At the outset, judges may perceive their role on the bench as quite different from that of party activist. Their understanding of the "rules of the game" may be such as to make party an improper consideration in decision-making. They may become responsive to new reference groups—such as the bar and the press—which hold the same view. The socializing influences of party may be weakened for the judge if he is isolated from party activists by his judicial duties. In addition, there are the new socializing influences of work in a collegial court; the expectations of the other members may alter the attitudes of the new judge. Also, there is the influence of task requirements, the need to get the court's work done, which may cause men to act in a way quite distinct from the ways in which they acted when the job of task fulfillment involved party activists and had quite different objectives. Finally, there is the nature of judicial business; the complexity of the concrete issues which are the ordinary business of the highest state courts may make applications of party-shaped policy orientations less clear and may introduce other sets of values for consideration.

An understanding of the judge's situation then becomes quite important in understanding the relationship between party and decisional propensity. His situation might continue to include a high degree of contact with party. This could easily occur in the selection, tenure and promotion process. Indeed, it is argued in Section VI of this paper that the degree of partisan influence in their constituencies is a significant variable in explaining the sharply different conduct of Wisconsin and Michigan supreme court justices in deciding cases. Furthermore, the effects of membership in a collegial court may depend on whether justices have extensive personal contacts in the disposition of their work. Section VII of this paper suggests that the existence of such contacts may make the conduct of Wisconsin justices less partisan than that of Michigan justices.

In summary, the effect of party on judicial behavior depends on an interplay of factors. Foremost is a complex of psychological factors, including party identification and perception of the judicial role, which shape judges' policy orientations. The socializing processes which result from situational circumstances—party activism and judicial work are especially pertinent in the present study—may alter or re-enforce policy orientations, either strengthening or diminishing the significance of party in judges' psychological sets. The situational circumstances may vary significantly from state to state and these socializing processes depend therefore on the nature of the circumstances in each jurisdiction.

In addition to the conceptual question of the link between party and judicial behavior, the studies of the party variable in judicial decision-making raise an important methodological

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Theory and Research (Chicago: Rand McNally, 1964), pp. 197, 200.

[28] Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," this REVIEW, 44 (June, 1960), 406-427. Thomas A. Flinn and Frederick M. Wirt, "Local Party Leaders: Groups of Like Minded Men," *Midwest Journal of Political Science,* 9 (February, 1965), 77–98. Also, Samuel J. Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally, 1964), pp. 184–185.

[29] McClosky, *et al., op. cit.,* p. 421.

[30] *Ibid.*

[31] *Ibid.*

problem: what test should be applied to judicial conduct to infer with reasonable certainty the existence of party influence? Students of legislative behavior have generally applied tests which emphasize the extent to which members of the same party vote together as a bloc. In his pioneering work on party influence in legislative bodies A. Lawrence Lowell described a "party vote" as one "in which more than nine-tenths of those of its members who voted were on the same side of the question."[32] Similarly, Stuart Rice's widely used index of cohesion measures the extent to which all members of a group vote together on an issue.[33]

To isolate issues on which party influence is significant Rice also developed his index of likeness between groups (often called the index of party likeness or IPL) which shows the extent to which members of different groups vote differently.[34] The familiar *Congressional Quarterly* test of party voting requires that a majority of voting Democrats oppose a majority of voting Republicans on an issue.[35]

The major studies of partisan influence in legislative bodies have employed these indexes, all of which emphasize bloc voting by party members either within a single party or by contrast between the two parties.[36] While some objection has been raised to the undiscriminating use of roll call data, especially to the IPL which might mask party influences where the leaders of both parties are in agreement or friendship influences where the party leaders take no positions,[37] it seems clear that such data, carefully used and qualified, can be helpful in suggesting the influence of party on legislative behavior.[38]

The recorded votes of judges in cases before their courts provide roughly the same kind of data that roll calls provide for the study of legislative bodies. Furthermore, the same criterion seems relevant for inferring party influence: that is, the extent to which judges of each party vote together as distinct blocs in opposition to one another. Where party identification is largely coterminus with a voting bloc, there is a strong likelihood that party plays a significant role in the voting. Research beyond the voting records may, of course, reveal other factors at work in both judicial and legislative bodies.

Although Schubert and Ulmer present some of their data in simple roll call type analysis (e.g. the percentage of votes by each judge for the worker in workmen's compensation cases), they also study the Michigan Supreme Court with scalogram and bloc analysis techniques. As indicated above, their findings show sharp cleavages on that bench between Democrats and Republicans acting as separate blocs.

Nagel and Goldman, on the other hand, employ techniques which, in the opinion of this author, yield far weaker inferences of party influence. Both regard a "liberal vote" by a Democratic judge in a given case as related to his party, even though other Democratic judges on the same bench in the same case are voting for a contrary disposition of the matter. If party affiliation were the primary variable, it should affect most or all of the members of the same party in approximately the same way. Where judges of the same party differ to a significant extent on the disposition of issues before their court, it is as plausible to infer that party, as previously conceptualized, is not a variable in the judicial process as it is to infer that a "liberal" or "conservative" vote by a judge is linked to party. In the face of equally plausible inferences, judges' conduct ought not to be explained by the party variable.

III. A WISCONSIN CASE STUDY: SETTING AND METHOD

The testing of the party variable with additional data seemed necessary since the Schubert and Ulmer studies showing with reliable techniques strong evidence of party influence in judicial voting rested on data from only a single state, Michigan. The use of Wisconsin data for this retesting was dictated by several factors. First was the impressionistic evidence that in

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[32] "The Influence of Party Upon Legislation in England and America," *Annual Report of the American Historical Association*, 1901 (Washington: Government Printing Office, 1902), Vol. 1, p. 323.

[33] *Quantitative Methods in Politics* (New York: Alfred Knopf, 1928), p. 209.

[34] *Ibid.*, pp. 210–211.

[35] See *Congressional Quarterly Weekly Report*, No. 49 (1966) p. 2989 for an explanation of the technique which CQ has consistently used in its congressional voting analyses.

[36] See Julius Turner, *Party and Constituency: Pressures on Congress* (Baltimore: The Johns Hopkins Press, 1955), pp. 27–29, 36–71. Also, David Truman, *The Congressional Party* (New York: John Wiley and Sons, 1959), esp. chs. 3, 5. And Duncan MacRae, Jr., "The Relationship Between Roll Call Votes and Constituencies in the Massachusetts House of Representatives," this REVIEW, 44 (December, 1952), 1046–1055.

[37] Wilder Crane, Jr., "A Caveat on Roll-Call Studies of Party Voting," *Midwest Journal of Political Science*, 4 (August, 1960), 237–249.

[38] Fred I. Greenstein and Elton F. Jackson, *op. cit.*

Wisconsin party did not play a key role in a series of highly visible and intensely partisan disputes before the Court in 1964 and 1965. Conflicts over the appointment power[39] and reapportionment[40] between Democratic Governor John W. Reynolds and Republican majorities in both legislative houses and a dispute between Democrats and Republicans in the state assembly over the seating by the Republican majority of a claimant to a lower house seat as against his Democratic rival whose election had been certified by the canvassing board and a lower court[41] were finally settled by the Wisconsin Supreme Court. In each case the decision was unanimous despite the close division on that Court between adherents of the two parties.

Second, one of the variables to be tested in this study is the extent of partisanship in judicial campaigns, and this required on-the-scene research. Wisconsin was convenient for this purpose. Third, there are significant similarities between the Wisconsin and Michigan political systems other than mere geographical proximity. The use of Wisconsin for further testing was a rough attempt at holding constant some variables in the political system. In both states, for instance, the parties are organized on a mass membership basis. In the post-war years, the Democratic party in both states was taken over by liberal political activists and was closely aligned with the labor movement. The Republican party in both states was dominated by activists of a clearly conservative ideology (at least until the early 1960's).[42] Financial support for the Democrats in both states was drawn primarily from union labor, liberal intellectuals, and a variety of professional and small business people, while the largest single source of Republican financial support was manufacturing and financial interests.[43] In short, both states were marked by close interparty competition between highly issue-oriented party organizations divided along contemporary liberal-conservative lines and financed from sources compatible with such a division.

There were also substantial similarities between the structural arrangements of the highest courts of the two states.[44] Each state, for instance, elects supreme court justices on a nonpartisan ballot for a fixed term, eight years in Michigan and ten in Wisconsin. Each selects judges in spring election rather than during the November balloting. And in both states the governor is empowered to fill vacancies. A striking difference is found in the nominating process, which in Michigan is by party convention and in Wisconsin by nonpartisan primary.

Another similarity between the two states was the changing membership of their supreme courts. In 1954, the Michigan Supreme Court was composed of six Republicans and two "old line" conservative Democrats.[45] By 1958, the process of appointment and election had converted the Court's membership to five "Williams" or liberal Democrats and three Republicans. The Wisconsin Supreme Court began with a division of six Republicans and one Democrat in 1957 and was altered by appointments and elections to a division of four Democrats and three Republicans by 1965. The Michigan studies show that a change in judicial conduct accompanied the change in party division on the Court. Since Wisconsin underwent similar alteration of the party division in a quite similar party environment, Wisconsin seems an appropriate place to attempt a validation of the party variable hypothesis.

The period studied here is from January 7, 1957, to January 1, 1966. During that time nine Republicans and four Democrats held posts on

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[39] *State ex rel. Thompson v. Gibson et al.,* 22 Wis. 2d 275 (1964); *State ex rel. Reynolds v. Smith,* 22 Wis. 2d 516 (1964).

[40] *Stat ex rel. Reynolds v. Zimmerman et al.,* 22 Wis. 2d 544 (1964); *State ex rel. Reynolds v. Zimmerman,* 23 Wis. 2d 606 (1964).

[41] *State ex rel. Elfers v. Olson,* 26 Wis. 2d 422 (1965).

[42] The ideological and programmatic aspects of the parties are described in John Fenton, *Midwest Politics* (New York: Holt, Rinehart and Winston, 1966), chs. 2–3; Stephen B. and Vera Sarasohn, *Political Party Patterns in Michigan* (Detroit: Wayne State University Press, 1957); Leon D. Epstein, *Politics in Wisconsin* (Madison: University of Wisconsin Press, 1958), pp. 36–38, 46–56, 82–86; and Frank Sorauf, "Extra-Legal Political Parties in Wisconsin," this REVIEW, 48 (September, 1954), 692–704.

[43] Michigan party finance is thoroughly described in John P. White and John R. Owens, *Parties, Group Interests and Campaign Finance: Michigan '56* (Princeton: Citizens' Research Foundation, 1960). The financial bases of the Wisconsin parties are described in Epstein, *op. cit.,* pp. 79–80; H. Gaylon Greenhill, *Labor Money in Wisconsin Politics, 1964* (Princeton: Citizens' Research Foundation, 1966), esp. pp. 22–39; and David Adamany, *Money in a State Political System: Wisconsin* (unpublished Ph.D. dissertation, Department of Political Science, University of Wisconsin, 1967), esp. ch. 5.

[44] Information about the selection of judges in Michigan is derived from S. Sidney Ulmer, *op. cit.,* unless otherwise noted.

[45] *Ibid.,* p. 268.

TABLE 1. MEMBERSHIP CHANGES AND PARTY DIVISION ON THE WISCONSIN SUPREME COURT: JANUARY 7, 1957 TO JANUARY 1, 1966

Period		I	II	III	IV	V	VI	VII
Time Span:	From	Jan 7, 1957	May 1, 1958	Jan 5, 1959	Jan 2, 1962	June 5, 1962	Jan 6, 1964	Aug 18, 1964
	To	Apr 30, 1958	Jan 4, 1959	Jan 1, 1962	June 4, 1962	Jan 5, 1964	Aug 17, 1964	Jan 1, 1966
Republicans		6	6	6	5	4	4	3
Democrats		1	1	1	2	3	3	4

the Wisconsin Supreme Court. Six of the nine Republicans and two of the four Democrats went to the bench initially upon the appointment of governors of their respective parties; the remainder obtained their offices through election. Of the nine justices with Republican affiliations, six had sought elective office on the party ticket, two had served in appointive positions in a Republican gubernatorial administration, and the ninth had no history of active partisanship. Two of the four Democratic-affiliated justices had campaigned and been elected to office on the party ticket, one had served in high party office and as an appointee in a Democratic administration, and the fourth had been active as an advisor and contributor to several prominent Democratic leaders. All four had been dues-paying members of the Democratic Party. Twelve of the thirteen justices had therefore engaged in the kind of party activity which should have exposed them to the influence of party as conceptualized in this paper.

It is difficult to analyze the degree of partisanship in the Court's deliberations during a particular calendar year or Term of Court because of changes in membership on the bench. Judicial tenure begins in January, and Terms of the Court begin in August. There is no fixed period of stable membership on the court because appointments are made whenever a vacancy occurs through death or resignation. For purposes of analysis, the nine year period under study was therefore divided into seven periods, each representing the time during which the same seven justices served together. Whenever membership on the Court changed, a new period began. The length of these periods varies widely, the shortest lasting only six months and the longest almost three years. Table 1 reports the seven time periods and the partisan division on the Court during each.

IV. WORKMEN'S AND UNEMPLOYMENT COMPENSATION CASES

Both Schubert and Ulmer used workmen's compensation cases to test the relationship between party affiliation and judicial conduct in Michigan, and Ulmer used unemployment compensation cases as well. These policy areas are also well suited to testing the party variable on the Wisconsin Supreme Court because, as in Michigan, the contesting interests in these cases are clearly identified with different parties. The business community has traditionally been tied to a conservative Republican Party organization and the labor movement to a liberal Democratic Party. These issues thus invite policy divisions along party lines.

Although simple roll call techniques do not show party bloc cohesion on issues, Table 2 is a roll call statement of the voting of Democratic and Republican judges on the Wisconsin Supreme Court on workmen's compensation cases for the seven periods here under study. This table replicates with Wisconsin data Ulmer's presentation of the voting records of Michigan judges on this issue. Replication is useful here because it shows that, even by the least rigorous cirterion, the sharp party line division of judges which occurred in Michigan on this issue is not found in Wisconsin.

The Ulmer data show that in no Term of Court from 1954 to 1960 did any Republican justice vote for the claims of injured workers as frequently (measured by persentages) as the Democratic justice with the record least favorable to the worker-claimants. The narrowest difference between the most pro-worker Republican justice and the least pro-worker Democratic justice was nine percent in 1958 and the widest margin was 36 percent in 1960. The average difference between the two parties for the six Terms studied was 23 percent.

A note of caution is required about the Wisconsin data prior to their comparison with those from Michigan. There appears to have been a number of instances of nonparticipation which affected the pro-worker scores of two Democratic justices. Justice Fairchild and Justice Wilkie disqualified themselves in cases which had originated in lower courts presided over by their father and brother, respectively. More instances of disqualification occurred in cases decided for the worker without dissent than in cases which divided the Court, causing certain of the Fairchild and Wilkie pro-worker percent-

TABLE 2. PERCENTAGE OF VOTES IN FAVOR OF WORKMEN'S COMPENSATION CLAIMANTS: WISCONSIN SUPREME COURT, 1957–1966**

Period	I	II	III	IV	V	VI	VII
Number of Cases	11	15	39	4	7	3	16
Heffernan (D)							56.2
Wilkie (D)					57.1*	33.3*	43.8*
Gordon (D)				25	85.7	66.6	50
Fairchild (D)	63.9*	40*	33.3	25	100	66.6	56.2
The Court	*90.9*	*60*	*33.3*	*25*	*85.7*	*66.6*	*56.2*
Currie (R)	90.9	60	33.3	25	71.4	66.6	56.2
Hallows (R)		53.3	30.7	25	85.7	33.3	43.8
Brown (R)	90.9	26.6*	33.3	25	85.7		
Broadfoot (R)	100	53.3	33.3	25			
Martin (R)	90.9	33.3*	30.7*				
Dieterich (R)			35.8	25	100	66.6	
Beilfuss (R)						66.6	50
Steinle (R)	81.8						
Wingert (R)	81.8	60					

R = Republican; D = Democrat.

* These percentages are affected by nonparticipation in some workmen's compensation cases decided by the Court during the Period described. See explanatory discussion in the text.

** This Table adopts the format used by Ulmer to facilitate comparison of these Wisconsin data with those he developed for Michigan.

ages to be somewhat lower despite the fact that they cast no more anti-worker votes than their brethren. Occasional nonparticipation by other justices, usually because of illness, affected their scores, although not to the same extent. The scores affected by nonparticipation are designated in Table 2 with an asterisk.

The notable feature of Table 2 is the lack of difference along party lines in the pro-worker scores of the justices. In each of the seven periods, Republican justices made records as favorable to the worker as their Democratic counterparts. A possible exception is Justice Hallows (R) who was somewhat less favorable to the worker-claimants than his brothers of both parties. Although a Republican, Justice Hallows came to the Court with a much less party activist background than that of most justices, and so even in his case it is not clear that party was a significant variable.

More impressive than this type of roll call analysis was Ulmer's finding that in the 1958–1960 Terms of the Michigan Supreme Court there was clear bloc voting along party lines on workmen's compensation and unemployment compensation cases. He found three patterns of voting: all justices voting for the worker's claim, all justices voting against the worker's claim, Democratic justices (with an occasional deviation) voting for the claim and Republican justices voting against it.[46] Where division on a case did occur, it always featured Democratic justices taking the pro-worker position and Republican justices taking the anti-worker position; this pattern looks very much like the cleavages between cohesive party blocs found in some analyses of legislative behavior.

Tables 3 and 4 report the votes of Wisconsin Supreme Court justices on workmen's and unemployment compensation cases from 1957 to 1966.

Although laid out in scalogram form for convenience, the data cannot be tested for scalability because of the large number of nonparticipations. The tables show, instead, the lack of party bloc voting on the same kinds of issues which sharply split the Michigan court, operating in a similar political environment, along party lines. The left hand column in each table records the number of times each justice cast a vote for the worker's claim in a case decided without dissent. Similarly, the right hand column records the number of times each justice cast a vote against the worker's claim in a case decided without dis-

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[46] *Ibid.*, pp. 360–361.

TABLE 3. VOTES IN WORKMEN'S COMPENSATION CASES: WISCONSIN SUPREME COURT: 1957-1966

	+ without dissent	6–1	6–1	6–1	6–1	5–1	5–1	5–2	5–2	2–4	1–6	1–6	– without dissent
Dieterich (R)	19		+	+		+				+		+	28
Broadfoot (R)	29	+	+				–	+			+	–	34
Fairchild (D)	36	+	+	+	+	+	+	+	+	+	–	–	38
Currie (R)	42	+	+	–	+	+	+	+	+	–	–	–	42
Gordon (D)	13			+	+	+			+	–			9
Wilkie (D)	9			+	+	NP			+	NP			6
Heffernan (D)	7				+				+				7
Wingert (R)	15	+					+	+			–		6
Martin (R)	24	+	+				+	–			–	–	27
Beilfuss (R)	8				+	+			–				8
Brown (R)	31	+	+	+			NP	–		–	–	–	30
Hallows (R)	32		–	+	–	–	+	+	–	–		–	42
Steinle (R)	9	–									–		
N = 95													

sent. The description of a case as "decided without dissent" encompasses some cases in which one of the justices did not vote. The cases presented individually are those on which the Court divided.

Two aspects of the data presented in Tables 3 and 4 distinguish judicial voting patterns on the Wisconsin Supreme Court in workmen's and unemployment compensation cases from the patterns found in Michigan. First, the frequency of disagreement on the Wisconsin Court was much less than on the Michigan Court. Of the 95 workmen's compensation cases decided by the Wisconsin Supreme Court during the period under study, 88 percent (84 cases) were decided without dissent. Schubert's data for the 1954–1957 Terms of the Michigan Court are not presented in a manner which permits the computation of the percentage of workmen's compensation cases on which that court divided, but Ulmer's data show that the Michigan justices decided only 64 per cent (19 of 29 cases) of the workmen's compensation matters without dissent during the 1958–1960 Terms. Of the unemployment compensation cases, the Wisconsin justices decided 68 per cent (13 of 19 cases) without dissent, while their Michigan counterparts decided only 50 per cent of (6 of 12 cases) without dissent. The greater percentage of cases decided without dissent by the Wisconsin Supreme Court suggests that party conflict did not play the major role in that Court's deliberations that it did in the Michigan Court, even though the party systems in both cases were characterized by sharp polarization of labor and management interests into program-oriented Democratic and Republican parties respectively.

TABLE 4. VOTES IN UNEMPLOYMENT COMPENSATION CASES: WISCONSIN SUPREME COURT: 1957–1966

	+ without dissent	6–1	6–1	5–2	5–2	3–4	3–4	– without dissent
Fairchild (D)	5	+	+	+	+	+	+	8
Dieterich (R)	2	+				+	+	4
Brown (R)	2	+		+	+	+	–	4
Wingert (R)	1			+	+			2
Martin (R)	1	+		+	+	–	–	3
Currie (R)	5	+	+	–	–	–	–	8
Gordon (D)	4		+					4
Wilkie (D)	3		+					2
Heffernan (D)	2		+					2
Beilfuss (R)	3		+					4
Broadfoot (R)	1	+		–	–	–	+	4
Hallows (R)	4	–	–	+	+	–	–	7
Steinle (R)	0							1
N = 19								

A second aspect of the data in Tables 3 and 4 raises an even stronger inference that party affiliation was not a significant factor among Wisconsin Supreme Court justices in these two policy areas. In none of the 17 split decisions in the workmen's and unemployment compensation fields did the Democratic justices vote as a bloc on one side of the issue and the Republicans as a bloc on the other. This was, by contrast, the predominant pattern in the split decisions of the Michigan Supreme Court. In 13 of the 17 split decisions in Wisconsin a majority of the justices affiliated with each of the parties joined to make the Court majority. None of the more numerous Michigan split decisions brought together a ma-

TABLE 5. DISAGREEMENT ON THE WISCONSIN SUPREME COURT: 1957–1966

Time Period	Justices Dem. N	Justices Rep. N	Total Decisions	Split Decisions	Dissenting Votes	Ratio of Split to Total Decisions	Average No. of Dissenting Votes Per Split Decision
I	1	6	305	43	79	.14	1.84
II	1	6	210	30	43	.14	1.43
III	1	6	727	68	117	.09	1.72
IV	2	5	113	14	22	.12	1.57
V	3	4	382	67	115	.18	1.71
VI	3	4	182	27	45	.15	1.67
VII	4	3	342	51	98	.15	1.92

jority of each party to make a Court majority; the divisions all reflected sharp cleavages between tightly knit party blocs.

V. PARTY AFFILIATION AND DISSENTING BLOCS

In addition to sharp party-line divisions on workmen's and unemployment compensation, the Michigan Supreme Court was also characterized by a clear pattern of dissenting bloc voting along party lines. At the outset, this kind of analysis is open to some question. Unlike the workmen's and unemployment compensation issues, the total agenda of judicial business does not consist of matters on which the parties are naturally divided. Notwithstanding this qualification, Schubert's evidence shows such a high degree of division along party lines on the general business of the Michigan Supreme Court that he raises a reasonable inference that party affiliation was a significant variable which pervaded most of that Court's deliberations.[47]

At the outset, Schubert found in the overall business of the Michigan Court "a rising curve of disagreement" which was illustrated by a rising ratio of split to total decisions and an increase in the average number of dissenting votes per split decision.[48] ". . . [T]he infusion of the Williams Democrats," he argued, "led directly to a considerable increase (260 per cent) in disagreement among the justices."[49]

Similar analysis of the dissenting votes cast in all cases decided by the Wisconsin Supreme Court during the rise of its Democratic membership from a minority of one to a majority of four does not reveal such "a rising curve of disagreement." Table 5 reports the data for Wisconsin.

In Michigan the ratio of split to total decisions rose from .09 for the 1955 Term in which only one Democratic justice served on the Court to .16 for the 1957 Term in which the Court was divided evenly between the parties. By contrast, the ratio in Wisconsin was about the same in the early periods (.14) when Justice Fairchild was the lone Democrat and in the later periods (.15) when the Court shifted from a four to three Republican majority to a four to three Democratic majority. The average number of dissenting votes per split decision on the Michigan Court increased steadily from 2.33 to 2.95 from 1955 to 1957. In Wisconsin the average was 1.84 in the early period and 1.92 in the last period, with the intervening periods showing a fluctuation among somewhat lower averages.

It is significant that the average number of dissenting votes per split decision did not rise in Wisconsin as the membership of the Court became more evenly divided between justices of different party backgrounds. Party as a significant determinant of judicial voting on the general business of the Court should be shown by an increase in the average number of dissenting votes per split decision in such circumstances because the minority party, acting as a bloc, would have more votes to cast in dissent in cases which divided the Court. The absence of such an increase suggests the absence of party as an important variable in voting on the Wisconsin Supreme Court.

The second phase of Schubert's investigation of party affiliation as a determinant of division on the Michigan Supreme Court consisted of dissenting bloc analysis for the 1954–1957 Terms of that Court. Schubert developed the Index of Cohesion for describing the "average frequency of dissenting votes cast by bloc members in conjunction with other members of the same bloc."[50] An Index of Cohesion of .50 or greater is considered high, an Index of Cohesion

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[47] Glendon Schubert, *Quantitative Analysis of Judicial Behavior,* pp. 135–142.

[48] *Ibid.,* p. 134, esp. Table 3.15.

[49] *Ibid.,* p. 134.

[50] *Ibid.,* p. 89.

TABLE 6. EXISTENCE AND COHESION OF DISSENTING BLOCS ON THE WISCONSIN SUPREME COURT: 1957–1966

Time Period	Justices		Blocs by Index of Cohesion		
	Dem. (N)	Rep. (N)	Low	Moderate	High
I	1	6	Currie (R)–Steinle (R)–Wingert (R) (.30)	Currie (R)–Fairchild (D) (.43)	
II	1	6			
III	1	6		Martin (R)–Broadfoot (R) (.46)	
IV	2	5	Currie (R)–Dieterich (R)–Hallows (R) (.31)		Fairchild (D)–Brown (R)–Gordon (D) (.50)
V	3	4	Wilkie (D)–Hallows (R)–Currie (R) (.39)	Dieterich (R) Gordon (D) (.44)	
VI	3	4	Wilkie (D)–Gordon (D)–Fairchild (D) (.33) Hallows (R)–Currie (R)–Beilfuss (R) (.32)		
VII	4	3	Hallows (R)–Currie (R)–Beilfuss (R) (.30)	Hallows (R)–Beilfuss (R) (.44)	

Indexes of Cohesion: Low = .30 – .39; Moderate = .40 – .49; High = .50 or higher. R = Republican; D = Democrat.

of .40–.49 is moderate, and any Index of less than .40 is treated as low.[51]

Schubert discovered distinct Democratic and Republican blocs, each with a high Index of Cohesion. In the 1955 Term, a "right bloc" of Republicans had an Index of Cohesion of .64. There was only a single Democrat, so obviously there was no "left bloc" pattern of voting. However, the lone Democrat did cast the largest number of solo dissents. In the 1956 Term, a "right bloc" consisting of four Republicans had a .55 Index of Cohesion and a "left bloc" consisting of the three Democrats had a .50 Index. In the 1957 Term, with the Court split evenly between four Democrats and four Republicans, the Indexes of Cohesion rose sharply, with the Democratic bloc achieving a .83 Index and the Republican bloc a .68 Index. In no instance during these three Terms were any Democrats and Republicans on the Michigan Supreme Court joined together in a bloc.

The pattern of judicial behavior in Wisconsin does not demonstrate this kind of partisanship. As Table 6 indicates, there were no stable blocs on the Wisconsin Supreme Court and such blocs as did exist in a given time period tended to be of low cohesion. Table 6 reports the existence on the Wisconsin Supreme Court of all dissenting blocs with a minimum Index of Cohesion of at least .30.

In Period I, Democratic Justice Fairchild and Republican Justice Currie constituted a moderately cohesive bloc, but Currie was also part of a three-man Republican bloc of low cohesion. In such circumstances, it must be concluded that party lines were almost non-existent, especially since the Court's three other members were Republicans and the existence of a three man Republican bloc would show some division within the party rather than along party lines. In Period IV a three-man Republican bloc of low cohesion and a three-man bloc of two Democrats and a Republican with a high cohesion index can be identified. In the next period, however, one of the members of the Republican bloc (Dieterich) and one of the Democratic members (Gordon) of the mixed bloc were found voting together with moderate cohesion. Meanwhile, Dieterich's place in the Republican bloc was filled by newly appointed Justice Wilkie, a Democrat.

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[51] *Ibid.*, p. 90.

In Periods VI and VII there are hints of partisanship among dissenting blocs. A three-man Democratic bloc and a three-man Republican bloc are clearly visible in Period VI, and the Republican bloc is again found in Period VII. The suggestion of partisanship might appear stronger with the report in Period VII of a three-man Democratic bloc (although one of the members has changed) with a cohesion index of .29, just below the .30 minimum level set for presentation in Table 6. It must be noted, however, that these blocs are characterized by very low cohesion scores which contrast them sharply to the cohesion scores of .83 (Democratic) and .68 (Republican) found in Michigan when its Court was closely divided between the parties.

A more cogent critique of interpreting these kinds of dissenting blocs as signs of party cohesion can be found in Joel Grossman's recent paper arguing that cohesion scores may be misleading as to the actual existence of bloc voting.[52] Grossman notes that high cohesion scores may be yielded when two members of a bloc frequently vote with a third member but not with one another. Furthermore, other justices may join bloc members in a number of dissents which count toward the cohesion score without themselves being counted as members of the bloc. Thus the apparent existence of a bloc may be refuted by a closer analysis of the actual frequencies with which various members vote together. This is especially relevant for analysis of partisan blocs whose existence is predicated on the assumption that members of each party act as a cohesive group whose objectives are contrary to those of the opposition party.

Table 7 is modeled on Grossman's presentation of the voting of members of blocs on the United States Supreme Court.[53] The presentation is designed to test more precisely than the cohesion index the actual degree of group voting by bloc members in Periods VI and VII when slight suggestions of party voting appear in the dissenting bloc analysis. It is obvious that Grossman's point is pertinent to the blocs found on the Wisconsin Supreme Court. Seldom were all members of the bloc found voting together; the more common pattern was for two of them to vote together, sometimes as a pair and at other times in conjunction with a non-bloc member. Furthermore, these non-bloc members were frequently members of the opposing party bloc. Thus in Period VII, all five instances in which a pair of Republican bloc members voted together as part of a larger group found them in alliance with a member of the opposing Democratic bloc. Similarly, a member of the Republican bloc provided the additional vote in the three cases in which a pair of Democrats voted together as part of a larger group.

TABLE 7. ACTUAL PATTERN OF JOINT DISSENT AMONG MEMBERS OF DISSENTING BLOCS: WISCONSIN SUPREME COURT

Bloc As Reported	Dissented Alone: Number of Times	Dissented as Part of Larger Group: Number of Times
Period VI		
Currie-Hallows-Beilfuss (all Republicans)	1	0
Currie-Hallows	2	0
Hallows-Beilfuss	2	0
Currie-Beilfuss	0	0
Fairchild-Wilkie-Gordon (all Democrats)	1	0
Fairchild-Wilkie	2	0
Fairchild-Gordon	1	0
Wilkie-Gordon	0	0
Period VII		
Currie-Hallows-Beilfuss (all Republicans)	1	0
Currie-Hallows	2	1
Hallows-Beilfuss	5	3
Currie-Beilfuss	0	1
Fairchild-Heffernan-Gordon (all Democrats)	2	0
Fairchild-Heffernan	0	1
Fairchild-Gordon	2	2
Heffernan-Gordon	0	0

Several different aspects of this dissenting bloc analysis add to our previous evidence that party was not a significant determinant of judicial behavior in Wisconsin. First, the addition of members to the minority party on the bench neither increased the frequency of division on the Court, nor increased the average number of dissenting votes in those cases on which the Court divided. Second, such dissenting blocs as existed were often bipartisan. Third, dissenting blocs tended to be highly fluid, changing membership from period to period. Fourth, such dissenting blocs as did exist were marked by low cohesion scores. Fifth, even in the last periods where such low cohesion blocs were apparently partisan in their composition, a close analysis reveals frequent joint dissent by bloc members and justices of the opposite party.

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[52] Joel B. Grossman, "Dissenting Blocs on the Warren Court: A Study in Judicial Role Behavior," (unpublished paper).

[53] *Ibid.*, p. 24.

VI. JUDGES, PARTISANSHIP AND CONSTITUENCY

The conceptualization of the role of party in judicial behavior which has been developed in this paper has emphasized the socializing processes which occur in certain situational circumstances. This section suggests that party continues to be an important reference group and socializing agency for judges who rely on party for election to and continued tenure on the bench. On the other hand, where judges' constituencies are essentially bipartisan, their situational circumstances are significantly different and party will be much less significant in the decisional process. The available evidence supports the view that the constituencies of Michigan supreme court justices are highly partisan and that those of Wisconsin supreme court justices are bipartisan. This would be an adequate explanation of the sharp difference in the role that party plays in the decisional process in those two states.

In both Michigan and Wisconsin, judges are elected on nonpartisan ballots in the spring elections; but that is an inadequate measure of the role of parties in judicial elections. Instead it is necessary to examine the actual forces at work in the election process for

> in some political settings nonpartisanship is essentially normal. Removing party labels from the ballot seems to have made little political difference, since local Republicans and Democrats are sufficiently strong to take over the nonpartisan elections and nominate and elect their own candidates.[54]

It is the actual relationship between judges and parties in elections, rather than the legal forms, that may affect the policy responses of judges.

One element of partisanship in Michigan elections not found in Wisconsin is nomination by party convention. This increases the likelihood that judges will see themselves as part of and will remain in active contact with political parties. In Wisconsin, where nomination is by nonpartisan primary balloting, the nominees need not view themselves as party candidates.

We do not have systematic data on the degree of partisanship in Michigan judicial elections, but Ulmer's brief description of the election process in that state stresses the influence of the party affiliations of judicial candidates.[55] Until 1939, Michigan nominated judicial candidates in party conventions and elected them on party ballots. In that year the state constitution was changed to make the ballot nonpartisan because "as the strengths of the two parties . . . approached a relative balance in state politics, competition for judicial office . . . involved that office in the basest kind of political struggles."[56] The party convention method of nomination was retained, however. Alteration of the ballot form did not, according to Ulmer, take party politics out of the election process. The press gives considerable coverage to the party convention during the nominating process and identifies candidates by party affiliation throughout the campaign. Furthermore, the candidates themselves recognize the partisan nature of both the election contest and the office of justice.[57] In at least one recent year, the Democratic candidates announced that they would campaign together as a party team after the nominating convention and warned their fellow partisans that an entire branch of the state government was up for capture in the judicial election.[58]

To compare Wisconsin judicial elections with the highly partisan judicial elections in Michigan, it is necessary to ascertain the extent to which these Wisconsin contests are in fact characterized by party-line divisions in the electoral and campaign supports given the candidates. To test the extent of partisanship in the campaign, the partisan affiliations of committee members, prominent endorsers, and campaign contributors are examined. The conduct of the newspapers, whose party affiliations can generally be identified, and of organized labor is also considered.

Although there were seven judicial elections between 1957 and 1966, the analysis of partisanship in the campaisn process will be restricted to the three which were fought between pairs of nominees who were clearly identifiable by past affiliation with opposing parties. These were the contests between Circuit Judge Myron Gordon, a Democrat, and former Republican attorney general Stewart Honeck in 1961; between Justice Horace Wilkie, a Democrat, and Howard Boyle, a conservative Republican, in 1964; and between Justice Nathan Heffernan, a Democrat, and Boyle in 1965.[59]

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[54] Fred I. Greenstein, *The American Party System and the American People* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), p. 57.

[55] Ulmer, *op. cit.*, pp. 354–355.

[56] *Ibid.*, p. 354. Despite the evidence in Ulmer's article and the advocacy of change by some justices, the Michigan Constitutional Convention did not alter the institutional arrangements for the election of supreme court justices. The responsibility for nominating judicial candidates remains in the hands of party conventions, *Michigan Statutes Annotated,* Section 6.1392, and the general election ballot maintains its nonpartisan form, *ibid.*, Section 6.1393.

[57] Ulmer, *op. cit.*, pp. 354–355.

[58] *Ibid.*, p. 334.

The Gordon-Honeck Campaign. Circuit Judge Myron Gordon's successful campaign for the Supreme Court appears to have been based on a bipartisan constituency. Gordon was able to announce a committee of 657 Milwaukee county lawyers, almost half of whom were Republicans. A committee of past bar association presidents supporting Gordon consisted of five known Republicans and four lawyers with no clear party identification. A similar committee of "well-known attorneys" whose names graced a series of newspaper advertisements and campaign letters included three Republicans, two Democrats and three independents. While his opponent received the endorsement of Chief Justice John Martin, a former Republican attorney general, Gordon was able to claim the endorsement of Justice Timothy Brown, also a Republican.

Among the state's 21 daily newspapers in cities of medium or large size, 16 can be counted normally Republican, two Democratic and three independent based on past endorsements for governor and U.S. senator. Twelve of the 16 Republican papers preferred Democrat Gordon over his Republican rival, as did both Democratic papers and one of the independent journals. Honeck drew a single endorsement, from a Republican paper, and five papers did not act or their action could not be ascertained from the available research sources.

Five committees supporting Gordon reported to the secretary of state's office aggregate spending of $33,451. Labor, which endorsed Gordon, accounted for $1450. Seventy-six contributors of $100 or more gave $11,196. Of these, seven Republicans gave $1796 and 18 Democrats gave $1900. The remaining 61 contributors could not be identified as known partisans. The list suggests, however, that most were Jews, and that the strategy of Gordon's fund-raising effort was to solicit his co-religionists rather than regular party contributors.

The election results gave Gordon a margin of 33,000 votes in a polling of almost 766,000. The correlation between his percentage of the vote by county and the percentage by county of the Democratic nominee for governor five months before was .23, too low to be regarded as significant.[60] A similar correlation between his opponent's vote and that of the Republican gubernatorial nominee was −.02, despite several attempts by his opponent to take advantage of his Republican party activity and affiliation.[61] Gordon's own comment aptly described the campaign: "Party politics played no part in the election. I had bipartisan support. I belong to no party; I am no longer a dues paying Democrat."[62]

The Wilkie-Boyle Campaign. Like the Gordon campaign, that of Justice Wilkie showed a high degree of nonpartisanship. Boyle announced that his campaign would "include a criticism of Wilkie's record as a justice, and in particular his vote with the majority of the high court not to ban Henry Miller's *Tropic of Cancer* as an obscene book in Wisconsin."[63] Wilkie responded by "marshalling . . . bipartisan support of powerful allies—the bar, the bench, law professors, politicians, labor, businessmen and the press."[64]

Wilkie's statewide lawyers and citizens committees numbered among their leaders 37 prominent Republicans, 22 Democrats and 11 independents or others. The local committees supporting Wilkie showed a similar balance.

> Wilkie's active workers were generally political and bar notables—men of high public office, particularly in the state bar, with years of state and

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[59] Other judicial elections were the unopposed re-election of Justice George Currie (R) in 1957; the election of William Dieterich (R) over sitting Justice Emmert Wingert (R) in 1958; the election of sitting Justice Harold Hallows (R) over Stewart Honeck (R) in 1959; the election of Circuit Judge Bruce Beilfuss (R) over Harry E. Larsen, a Democrat so nominal that a number of state Democratic party leaders had never heard of him, in 1963.

[60] The author is aware of the dangers of using aggregate vote data, especially for as large a political unit as the county. Unfortunately this is the only data available for one of the three elections. Cf. Austin Ranney, "The Utility and Limitations of Aggregate Data in the Study of Electoral Behavior," in Austin Ranney (ed.), *Essays on the Behavioral Study of Politics* (Urbana: University of Illinois Press, 1962), pp. 91–102.

[61] John Wyngaard, "Wisconsin Report: Non-Partisan Pretense in Judicial Elections is Being Exploded in State," *Janesville Daily Gazette,* March 17, 1961, p. 6.

[62] *Milwaukee Journal,* March 8, 1961, p. 8.

[63] Jack Ladinsky and Allan Silver, "Popular Democracy and Judicial Independence," *University of Wisconsin Law Review* (Winter, 1967), p. 148.

[64] *Ibid.,* p. 149.

national, partisan and nonpartisan political experience.[65]

Survey data showed that about one-third of Wilkie's active supporters were Republicans.[66]

Wilkie gained the endorsement of 13 of the 16 Republican dailies, both Democratic papers and two of the three independent papers. Boyle received no endorsements from these papers. Wilkie also gained the endorsement of labor. He received the endorsement of three retired chief justices and two retired justices, all Republicans. Nine of his former colleagues in the state senate, including five Republicans, publicly supported his candidacy. Democratic Governor Reynolds and former Governors Nelson (D), Kohler (R), and LaFollette (Prog.) recommended his election. The most unusual declaration was that of five of his fellow justices, three Republicans and two Democrats, who severely criticized Boyle's discussion of specific court decisions as improper and urged the election of Wilkie.

The Wilkie forces reported spending $33,451.[67] Sixty-four people gave sums of $100 or more, their total gifts amounting to $12,698. Among them were 16 Republicans who gave a total of $6398, of which $4998 came from a single source, and 31 Democrats who contributed a total of $4150. Labor's contributions amounted to $7600.

On election day, Justice Wilkie was returned to the high court with a margin of 67,000 votes in 1,046,000 cast. Survey data demonstrate the bipartisanship of Wilkie's electoral base. They show that among Republican identifiers, Wilkie received 16.7 per cent to Boyle's 9.5 per cent, the remainder either not voting, not answering or not remembering. Among Democrats, Wilkie received 10.4 per cent to Boyle's 9.9 per cent, while the independents gave Wilkie 12 per cent and Boyle 8.4 per cent.[68]

The Heffernan-Boyle Contest. In many ways, the 1965 Heffernan-Boyle contest was a rerun of the 1964 judicial election. Like Wilkie, Heffernan was a Democratic appointee attempting to retain his seat. The thrust of Boyle's campaign was that the Court was too liberal, that it was legislating, and that it was usurping the powers of the legislature and the people. To respond to this campaign Heffernan was able to rally many of the same forces that had assisted Wilkie.

Heffernan was endorsed by all 16 Republican dailies, both Democratic dailies, and by two of the three independent papers. His statewide committee included 64 known Republicans, 82 known Democrats and 63 members who could not be identified. His county committees were similarly nonpartisan. His campaign committee was headed by former Republican Governor Oscar Rennebohm, and he drew endorsements from sitting Justice Myron Gordon, a Democrat, and retired Chief Justices Timothy Brown and John Martin, both Republicans. Heffernan also had the endorsement of organized labor.

The various committees supporting Heffernan reported spending $36,575, of which $11,225 was contributed by labor and $11,369 by donors of $100 or more. Seventeen of these donors, accounting for $6009 (although one contributor gave $3774), were Republicans and 19, whose aggregate gift was $3835, were Democrats.

On election day, Heffernan won by a slim margin of 13,500 in a total vote of 739,000. As in previous elections, the voters did not appear to divide along party lines. Survey data show that Republicans preferred Heffernan by 20.8 per cent to Boyle's 18.5 per cent; Democrats followed suit with 14.9 per cent for Heffernan and 14.4 per cent for Boyle; independents gave Heffernan 18.7 per cent and Boyle 15.7 per cent.

The foregoing data provide considerable evidence that members of the Wisconsin Supreme Court may accurately view their constituencies as bipartisan and do not need to remain actively engaged with their parties as a method for election or re-election to the bench. One objection to these data is that, since a Democrat was the winner in each instance, the support of labor and of Democratic politicians does not demonstrate the willingness of these forces to ignore partisan considerations in judicial contests. In 1967, after the termination of the present study, Chief Justice George Currie, a Republican, was opposed for re-election by Circuit Judge Robert Hanson, a Democrat. Although Currie failed of re-election, he was able to muster labor's endorsement and financial help as well as the endorsements and assistance of United States Circuit Judge Thomas Fairchild, a Democrat, Fed-

[65] *Ibid.*, p. 156.

[66] *Ibid.*, p. 157.

[67] The discrepancy between this figure and the $56,000 figure cited by Jack Ladinsky and Allan Silver, *op. cit.*, p. 150, is due to their inclusion in the total of certain transfer payments from one committee to another which swelled the reported total but did not actually represent new campaign spending. A similar disparity occurs between their report (p. 153) and mine concerning the amount spent in the Heffernan campaign.

[68] These survey data and those used in the subsequent section on the Heffernan-Boyle campaign were collected by Ladinsky and Silver for their previously cited study. I am grateful to Professor Ladinsky for his courtesy and cooperation in making them available for use in the present paper.

eral District Judge John W. Reynolds, a former Democratic governor, Democratic Attorney General Bronson La Follette, former Democratic Lieutenant Governor Patrick J. Lucey, and a host of Democratic party leaders and lawyers. It seems, therefore, that judicial candidates with prior Republican affiliations, as well as those with Democratic backgrounds, can reasonably hope for the support of a broadly bipartisan campaign constituency.

On the basis of limited data about judicial elections in two states, constituency emerges as a variable which has relevance to the study of party affiliation as a determinant of judges' voting. The situations of judges in Michigan continue to include party as a strong force, either because judges act directly to retain the support of their highly partisan constituencies or because there is a continuing socialization which occurs through their contacts with party during campaigns and the expectations of such contacts during the interelection period. The situations of judges in Wisconsin diminish the significance of party; judges may act consciously to retain the support of their bipartisan constituencies or their decision making may merely reflect a more permissive situation in which party is not a consideration.

VII. A PROCEDURAL CONSIDERATION: OPPORTUNITY FOR ACCOMMODATION

One additional factor in the judicial processes of Michigan and Wisconsin must be mentioned: the procedural arrangements which may hinder or encourage accommodation of views among members of a judicial body. Where judges do not have partisan constituencies, the existence of such arrangements may diminish even further the likelihood of party orientation in judicial decision making. This seems to be the case in Wisconsin. On the other hand, an absence of institutional arrangements encouraging comity and compromise may heighten even further the tendency for judges' voting to divide along party lines in states where judicial constituencies are partisan.

One Wisconsin justice suggested that the existence of a resident court, with members living in Madison and spending their working hours in adjacent offices, may be an important reason for the sharp contrast in the degree of partisanship between the Wisconsin court and the Michigan court, which is non-resident. His reasoning points out that in the period between the assignment of cases and the initial conference, on the one hand, and the final conference, on the other, there is ample opportunity for the justice to whom the case has been assigned to consult informally with other members of the Court of both parties to try to find a common ground for decision, or at least to obtain as large a consensus as possible. This varies sharply from the Michigan practice in which justices of a non-resident court feel free to write opinions and circulate them to their colleagues even while the justice assigned the case is submitting his draft. Only when the opinions are actually signed by the judges does a majority opinion emerge.[69] Little opportunity for accommodation is provided by such an institutional framework.

A further arrangement which may affect the decisional process in the two states is the time limitation on dissent in Wisconsin. Michigan justices may write and submit opinions during a fairly extended period prior to the final conference. Their Wisconsin counterparts, on the other hand, meet at conference on Friday before a Tuesday decision day to review the draft opinion submitted by the justice assigned the case, and then to vote. If a justice wishes to dissent, even after the prior opportunities for accommodation, he must normally prepare his dissenting opinion by the following Tuesday, although delays are sometimes requested and granted.

These procedural arrangements provide opportunity for accommodation and may result in the reconciliation of conflicting viewpoints before the actual decision is rendered; or at least the time pressures may discourage dissent in some cases. Such factors will not eliminate the play of more important determinants, such as policy orientation or party or constituency, but the frequency and intensity of the impact of these determinants may be affected by the procedural arrangements which guide a court's decisional process.

VIII. CONCLUSION

The argument that prior party affiliation is a determinant of judicial conduct has normative implications of substantial importance. It suggests, for instance, that in jurisdictions which elect judges, the ballot should be partisan inasmuch as party label would accurately identify for voters the kinds of policies a judge is likely to make on the bench. Furthermore, a party variable would suggest that, even where elections are legally nonpartisan, both voters and campaign activists should choose candidates with whom they share a party preference since party affiliation is a good forecaster of judicial conduct. The combination of these normative implications and the standing requirements of scientific accuracy should cause the party variable in judicial behavior to be treated quite gingerly.

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[69] The procedure on the Michigan court is described by S. Sidney Ulmer, "Leadership in the Michigan Supreme Court," in Glendon Schubert (ed.), *Judicial Decision-Making* (New York: The Free Press of Glencoe, 1963), pp. 16–17.

Assertions of party influence in judicial decision making based solely on the association of judges' prior party identifications and their propensity to cast "liberal" or "conservative" votes are inadequate, both conceptually and methodologically. A conceptual framework for understanding the influence of parties in judges' decisions ought to give weight to the role of party identification and party activism in shaping policy outlooks. Situational factors such as family, socio-economic group, etc., influence the development of an individual's party identification which then becomes an important psychological factor in shaping his issue orientations. Party activism is a second situational factor, and one common among judges, which influences issue orientations, both because parties tend to recruit those who are in agreement with their general policy stances and because of the socializing process among party activists which tends to shape and re-enforce policy outlooks congruent with those of the party. Thus, through party identification and activism judges are influenced by party. However, a change in situational circumstances, such as that involved in becoming a judge, may invoke a new set of identifications and a new socializing process which affects policy outlooks. To understand the importance of party in judicial conduct it is therefore important to know the extent to which party remains salient for judges after they take office. The election process is one significant aspect of a judge's situation in which party may continue as an important reference group for him.

A second problem in the studies to date has been the adequacy of the methodological devices used to infer the role of party from the voting records of judges. The roll-call techniques of the legislative behavior studies might fruitfully be used by students of judicial behavior. Measures which emphasize the difference between members of the two parties acting as cohesive blocs more convincingly raise an inference of party influence than do techniques which show only the number of votes cast by a judge in a particular ideological or partisan direction.

Although the conceptual and methodological discussions in this study are general, the evidence used is limited to that adduced from a case study of judicial behavior in Wisconsin. This case study evidence is valuable because it occurs in a political system quite similar to the Michigan political setting which has been the basis for the best previous studies. In both states the justices had clear prior party identifications and were recruited from situations in which party was a significant factor. The Wisconsin evidence, however, shows none of the vigorously partisan conduct by the justices which is demonstrated by the Michigan studies. The different situational circumstances of judges in the two states is a reasonable explanatory variable. In Michigan, party continues to be an important factor in judicial behavior because of the partisanship of judicial constituencies and the absence of a resident court with procedural methods which encourage accommodation. In Wisconsin, the influence of party diminishes because judicial constituencies are bipartisan and the Court's procedures create a considerable personal interaction between the justices which maximizes the opportunities for accommodation and compromise.

Nevertheless, it must be recognized that the Wisconsin evidence in no way concludes the inquiry into the role of party in judicial behavior. It does, in my view, provide additional data which complement those arising from the Michigan study. The whole of the existing literature does not, however, establish a party variable in state judicial decision making. Its methods and conceptualization are for the most part not rigorous enough; the number of jurisdictions examined with adequate methods is insufficient; and the range of variables considered in most studies is too narrow. All that has been established is the need for a more careful conceptualization and more rigorous testing of a provocative hypothesis.

COMPARING POLITICAL REGIONS: THE CASE OF CALIFORNIA*

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One of the more fertile sources of data for systematic comparative political studies is in the regional differences that abound within political systems.[1] When such differences are large and politically significant, explaining them becomes intriguing and important, especially if their causes cannot be found in the more familiar classes of socioeconomic variables.[2]

Our purpose here is to present a preliminary analysis of a widely discussed intrasystem political difference—that between Northern and Southern California. This regional split has received particularly wide attention since the 1964 Republican primary, when Senator Goldwater's landslide majority in the South overcame his resounding defeat in the San Francisco Bay Area and insured his presidential nomination. Attempts to explain this pronounced regional variation have generated propositions about the political consequences of those social and economic conditions thought to be characteristic of Southern California. Since that area's most striking feature is its continuous rapid growth and economic development, many writers have been led to speculate that anxieties resulting from such changes lead to ultraconservative political preferences. These propositions are of considerable interest to students of politics, since neither economic growth nor its presumed attitudinal consequence is unique to Southern California, nor, for that matter, to the United States. We will examine various explanations for California's regional variation, with special emphasis on propositions about economic growth. Our data are from the 1960 Census, 1964 and 1968 election returns, and a series of statewide sample surveys conducted during the 1964 campaigns.[3] This article is not a comprehensive exploration of the problem; we have not, for example, systematically investigated historical evidence. But we have analyzed a good deal more current information than other writers on this subject and hence can advance the discussion beyond its present stage.

I. REGIONAL POLITICAL DIFFERENCES IN CALIFORNIA

The conservative tone of political life in Southern California is well known. Radical right organizations flourish in the area, which is the prime source of out-of-state rightist mail received by Congressmen and Senators.[4] But the

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[1] For a general statement of this theme see Sidney G. Tarrow, *Peasant Communism in Southern Italy* (New Haven: Yale University Press, 1967), pp. 10–11 and *passim*. Other examples of intrasystem comparisons include Juan Linz and Amando de Miguel, "Eight Spains," in Richard Merritt and Stein Rokkan (eds.), *Comparing Nations* (New Haven: Yale University Press, 1966); Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW, 60 (March, 1966), 73–82; and Norval D. Glenn and J. L. Simmons, "Are Regional Cultural Differences Diminishing?" *Public Opinion Quarterly*, 31 (Summer, 1967), 176–193.

[2] An example of an exercise very similar to ours is V. O. Key, Jr. and Frank Munger, "Social Determinism and Electoral Decision: the Case of Indiana" in Eugene Burdick and Arthur J. Brodbeck (eds.), *American Voting Behavior* (Glencoe: The Free Press, 1959), pp. 281–299.

[3] The survey data used in this article are from four sample surveys conducted by the Field Research Corporation's California Poll. These studies are more fully described in our "The Repeal of Fair Housing in California: An Analysis of Referendum Voting," this REVIEW, 62 (September, 1968), 753–769.

[4] This statement is based on one of the authors' observations while working in several offices on Capitol Hill. Twenty-four per cent of the letters received by one national magazine in a radical right letter-writing campaign were from Southern California. See James McEvoy, III "Letters from

prophets of these movements find a cool and limited reception in the Bay Area, a celebrated center of liberal and radical political action. The distinctive character of political life in each of the two sections is illustrated by the comparative success of two new parties formed for the 1968 elections. By the summer of 1968 the left-wing Peace and Freedom party had 32,261 registered voters in the Bay Area, compared to only 6807 for George Wallace's American Independent party. The story was different in the Los Angeles metropolitan area, which had 27,012 people registered in the Peace and Freedom party and 51,290 under Wallace's banner. The same regional division is found at the elite level. For example, seven of the nine appointed regents of the University of California from Southern California voted to dismiss Clark Kerr as President of the University, compared to two of the seven from Northern California.[5]

Electoral behavior, although it does not do complete justice to the richness of either area's characteristic political style, provides the most convenient and precise measure of the cleavage and will be used hereafter as an index of the dependent variable. In 1964 Lyndon B. Johnson received 66 per cent of the presidential vote in the San Francisco Bay Area and 55 per cent in Southern California. Proposition 14, which repealed all fair housing legislation in the state (and subsequently was declared unconstitutional by the United States Supreme Court), was opposed by 42 per cent of the voters in the Bay Area and only 31 per cent in the South. In the 1964 Republican primary Governor Nelson Rockefeller won almost two-thirds of the vote in the North but only about two-fifths in the South. Four years later the liberal Republican incumbent Senator Thomas H. Kuchel was denied renomination by an even bigger conservative vote in the South, despite his three-to-two victory in the Bay Area. In the fall of 1968 Hubert H. Humphrey carried the San Francisco region handily but lost Southern California and the state[6] Table 1 presents returns for these five contests for the two major metropolitan areas in each of the two regions: San Francisco-Oakland and San Jose in the North, and Los Angeles-Long Beach and San Diego in the South.[7]

The importance of region as an independent variable is further demonstrated in an analysis of Proposition 14 returns from all incorporated Bay Area and Southern California cities of more than 2500 population. By assigning a numerical value of three to each Bay Area city and a value of one to each town in Southern California, we were able to compute simple and partial correlation coefficients to measure the relationship between region and votes against Proposition 14. The simple correlation coefficient (Pearson's *r*) is .55. When the vote for Johnson and Pierre Salinger (the Democratic candidate for Senator), the percentage of the labor force employed in manufacturing, median family income, and percentage non-white population are all partialled out, the coefficient is still .39.

r = .55

The regional differences in the Republican primaries are somewhat larger than in the Proposition 14 vote and considerably larger than in the presidential balloting. This suggests that the split is greater among Republicans.[8] Other

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the Right: Content-Analysis of a Letter Writing Campaign" (Ann Arbor: University of Michigan Institute for Social Research, 1966), p. 16.

[5] *San Francisco Sunday Examiner and Chronicle*, January 22, 1967, p. 8.

[6] While Southern California's greater conservatism has been fairly persistent in the recent past, the size of the regional differences in voting returns has not always been as great as in 1964 and 1968. For example, only four percentage points separated the two regions in the 1960 presidential election.

[7] The eight counties of Southern California contain 58 per cent of the state's population. The Los Angeles-Long Beach Standard Metropolitan Statistical Area (Los Angeles and Orange Counties) itself accounts for 43 per cent of all Californians. The other six southern counties resemble these two socially, economically, and politically. We will include respondents from all eight counties when analyzing survey data. With Census data and voting returns, it will be more useful to examine separately the two major metropolitan areas in the South: Los Angeles-Long Beach and San Diego.

We will compare Southern California not to the rest of the state, but just to the San Francisco Bay Area, which has 24 per cent of California's residents. With survey data, we define the Bay Area as one seven-county unit. For purposes of aggregate data analysis, we divide it into the San Francisco-Oakland SMSA and the San Jose SMSA. The reasons for this procedure will be apparent later in the article.

[8] Totton J. Anderson and Eugene C. Lee, "The 1964 Election in California," *The Western Political Quarterly*, 18 (June, 1965), p. 467. Fully a third of Bay Area Republicans intended, in late October, to vote for Johnson, compared to a 24 per cent defection rate among Southern California Republicans. The difference between the regions in Democratic defection was the same (but in the opposite direction): four per cent and thirteen per cent, respectively.

TABLE 1. VOTING RETURNS FOR SELECTED CALIFORNIA METROPOLITAN AREAS

	Northern California		Southern California	
	San Francisco–Oakland	San Jose	Los Angeles–Long Beach	San Diego
1964				
Per cent for Johnson	67	63	56	50
Per cent "No" on Proposition 14[a]	41	47	31	30
Per cent for Rockefeller	62	61	39	45
1968				
Per cent for Humphrey[b]	57	51	47	39
Per cent for Kuchel[c]	59	60	39	39

[a] "No" on Proposition 14 was a vote *for* fair housing.

[b] Per cent of the two-party vote. Percentages in this line were computed from the unofficial returns compiled by the Associated Press and published in the *San Francisco Chronicle*, November 7, 1968, p. 16.

[c] Per cent of all votes cast for Republican senatorial candidates. Both Senator Kuchel and his successful opponent, Max Rafferty, are from Southern California.

data point to the same conclusion: 44 per cent of Bay Area Democrats and 32 per cent of those in the South planned to vote against Proposition 14; the corresponding figures for Republicans are 33 per cent and 14 per cent, respectively.[9]

II. HYPOTHESES ABOUT SOUTHERN CALIFORNIA'S CONSERVATISM

We begin by noting (in Table 2) an array of demographic characteristics that do not distinguish between the two regions, or that differ in such small and inconsistent ways that they need not be referred to in the remainder of this article. Educational, occupational, and income levels

TABLE 2. POPULATION CHARACTERISTICS OF THE FOUR METROPOLITAN AREAS[a]

	San Francisco–Oakland	San Jose	Los Angeles–Long Beach	San Diego
Median family income in 1959	$7092	$7417	$7066	$6545[b]
Per cent in white collar occupations	51	51	49	50
Median school years[c]	12.1	12.2	12.1	12.1
Per cent aged 65 or older	9	7	9	7
Per cent foreign stock[d]	30	27	27	20
Per cent Negro	9	1	7	4
Per cent Oriental	4	2	2	1
Per cent Mexican-American[e]	6	12	9	6
Per cent white "anglo"[f]	82	85	82	89

[a] All data are from the 1960 Census.

[b] There was a considerable slump in the aerospace industry, San Diego's main source of manufacturing employment, in the late 1950's.

[c] For persons 25 or older.

[d] Persons born abroad or with one or both parents born abroad.

[e] The source for this line: U.S. Bureau of the Census, *U.S. Census of Population*: 1960. *Subject Reports. Persons of Spanish Surname. Final Report PC*(2)-1*B*. The Census Bureau estimates that virtually all people in California with Spanish surnames are of Mexican descent. Twenty percent were foreign born, 34 per cent native-born of foreign or mixed parents, and 46 per cent were native-born of native parents.

[f] People who are not Negroes, Orientals, or of Mexican descent.

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[9] These data, from the California Poll, are limited to white gentiles who had made up their minds how to vote by late October.

are strikingly similar, as is the proportion of the population over 65 (in spite of the emphasis in some popular writings on Southern California's retired folk as a source of its political tone). The various indicators of ethnic composition—per cent foreign stock, per cent Negro, per cent Oriental, per cent Mexican-American, per cent white "anglo"—do not differ in ways that suggest even impressionistically satisfactory explanations for the kinds of political variations reported in Table 1.[10]

We now turn to the proposition, advanced with varying degrees of explicitness by numerous scholarly and popular writers, that a major key to the regional political variation is Southern California's explosive population growth and its socio-political consequences. A number of observers have noted the prevalence of right-wing political sentiment in Southern California on the one hand and the area's phenomenal expansion on the other, and concluded that the latter explains the former. These propositions about California take on added significance in that the state is commonly said to foreshadow trends in much of the rest of the country; they often are closely related to more general ideas about the sources of various contemporary political movements which seem to be particularly popular in Southern California.[11]

Southern California's population explosion, which has been in process for all of this century and shows no signs of abating, is said to produce various manifestations of right-wing behavior for several different reasons. These can be divided into propositions referring to the preferences of individual citizens in the aggregate, which we will call "mass propositions," and ones referring to community leaders, political activists, and, in general, richer and better-educated people. For shorthand purposes we will call the latter "elite propositions."

One familiar mass proposition suggests that the new arrivals who continue to flood into Los Angeles and vicinity are lonely, bewildered, and isolated. Having left communities in which they had roots, unable to establish meaningful new social relationships, frustrated by the absence of familiar institutions, they turn in their trauma to the certainties of patriotism and political fundamentalism. In this view it is the newcomers whose votes and activity give Los Angeles its conservative cast. Seymour M. Lipset has presented as evidence for this proposition the finding from a 1962 California Poll that 39 per cent of the one hundred Californians who said they supported the John Birch Society had lived in the state for less than fifteen years, compared to 29 per cent of the respondents who opposed it.[12] Perhaps because of the size of the subsample of Birch supporters, he did not control for region, an important consideration because Southern California has more Birch supporters and more newcomers than Northern California.

None of our 1964 data support the proposition that people new to California are more likely to be conservative. Statewide, 27 per cent of white Republicans who had lived in California at least fifteen years preferred Goldwater to all other Republican presidential candidates, compared to 24 per cent of the more recent arrivals.[13] Controlling for region produced no meaningful relationship; newcomers to Southern California were slightly (six per cent) *less* likely to be for Goldwater.

The presidential preferences of brand-new Californians in the general election can be learned from the votes of "new residents", which are counted separately.[14] In Southern California there was no difference between the voting of new residents and all other voters: both groups gave 55 per cent of their votes to Johnson. In the Bay Area 60 per cent of the new voters and 66 per cent of the older residents were for Johnson. This does not give much support to the proposition and clearly does not help explain the regional difference.

Our survey data show that recent arrivals

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[10] While there is a slightly larger proportion of persons of foreign stock in the Bay Area, this difference is compensated for by the bigger Jewish community in Los Angeles.

[11] See, for example, Daniel Bell, "The Dispossessed—1962," in Bell (ed.), *The Radical Right*, Anchor edition (Garden City, N.Y.: Doubleday, 1963), p. 24; Seymour M. Lipset, "Three Decades of the Radical Right: Coughlinites, McCarthyites, and Birchers—1962," in *ibid.*, pp. 373–446; Lipset, "Beyond the Backlash," *Encounter* (November, 1964), pp. 11–24; Michael Miles, "Reagan and the Respectable Right," *The New Republic* (April 20, 1968), pp. 25–28; Talcott Parsons, "Social Strains in America: A Postscript—1962," in Bell, *op. cit.*, p. 234; Samuel C. Patterson, "The Political Cultures of the American States," *Journal of Politics*, 30 (February, 1968), p. 203; and Theodore H. White, *The Making of the President 1964* (New York: Atheneum, 1965), pp. 116–117.

[12] Lipset, "Three Decades," *op. cit.*, p. 437.

[13] Respondents were given their choice of all Republican presidential contenders, not just Rockefeller and Goldwater.

[14] "New residents" are people who have lived in the state for less than a year, were qualified to register in their previous state, and would be eligible to vote in California but for their recent arrival. They can vote only for President and Vice President. They probably are more interested in politics than the common run of registered voters.

TABLE 3. POPULATION AND POPULATION GROWTH IN THE FOUR METROPOLITAN AREAS

	San Francisco–Oakland	San Jose	Los Angeles–Long Beach	San Diego
1960 population	2,783,359	642,315	6,742,696	1,033,011
Increase in population, 1950–1960	24%	121%	54%	86%

to California have slightly more liberal attitudes toward Negro neighbors, particularly in the Bay Area. But there were no differences on attitudes toward the Rumford Fair Housing Act, which precipitated Proposition 14, or on voting intentions on the Proposition itself.

Californians characteristically move around a great deal from one residence to another. Perhaps this intrastate mobility contributes to those insecurities that presumably lead to ultraconservatism. If so, one might expect people who had lived in their present homes for a while to be less conservative. Our data do not support this idea either. White Republicans who had moved within the past five years were *less* likely (22 per cent to 30 per cent) to prefer Goldwater among all Republican presidential candidates. There was no difference between the two groups in attitudes toward the Rumford Act.

A second mass proposition about the effects of population growth is the reverse of the first one: the anxieties and hostilities caused by rapid, prolonged growth are felt more by the older residents, who resent not only the physical dislocations produced by expanding population, new traffic patterns, and changing land use, but also the social adjustments resulting from an influx of people of different cultural tastes, economic status, and, perhaps, ethnicity. The findings on newcomers and old-timers described above are faintly kinder to this proposition than to the first one, but given the small differences, these data do not support the argument that such outraged old settlers are the seedbed of Southern California conservatism.[15]

A third mass proposition holds that the "tensions of population growth" impinge on residents of booming areas irrespective of how long they have lived there. Since many of the social, geographic and economic consequences of rapid growth are felt generally by the population, their political corollaries may also be. Perhaps the association between rapid growth and conservatism is manifested in the impact of the supposedly fragmented social system in such areas on individual residents. To test this proposition one should compare all residents of rapidly growing areas with those of other areas, without distinguishing them as to duration of residence.

We can do this more satisfactorily with aggregate data than survey data. As Table 3 shows, the Los Angeles-Long Beach metropolitan area (Los Angeles and Orange Counties) grew more than twice as fast as the San Francisco-Oakland metropolitan area during the 1950's; and the greater conservatism of the Los Angeles region is clear.

But the relationship between growth and conservatism vanishes when we look beyond the two major urban centers to the pair of smaller metropolitan areas in the South and North. The San Diego metropolitan area, consisting of San Diego County, had a 1960 population of just over a million people. Despite a slump in its aerospace industries during the latter part of the decade, its population almost doubled from 1950 to 1960. The San Diego area's ten-year population increase was 86 per cent, considerably greater than the 54 per cent growth of the Los Angeles area. San Diego also exceeded Los Angeles in enthusiasm for Senator Goldwater and Proposition 14, as Table 1 shows.

During the same decade, however, the Northern California city of San Jose and its surrounding metropolitan area (Santa Clara County) grew at an even faster rate than San Diego. The area's population increased by 121 per cent from 1950 to 1960, yet Santa Clara County voted like the rest of the Bay Area, giving 63 per cent of its vote to Johnson, 61 per cent to Rockefeller, 60 per cent to Kuchel, and voting 47 per cent "no" on Proposition 14. Thus conservatism does not appear to be associated with massive population growth.[16]

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[15] A related proposition is that various recent manifestations of ultraconservatism reflect resentment by people from small towns and farms at the eclipse of older, simpler virtues and verities by the complex, cosmopolitan big city (see Lipset, "Beyond the Backlash," *op. cit.*, p. 20; and Bell, *op. cit.*, *The Radical Right, passim.*). Despite its popularity, no data have been presented to support this proposition and systematic empirical research on followers of the radical right provides no support for it. See Raymond E. Wolfinger, Barbara Kaye Wolfinger, Kenneth Prewitt, and Sheilah Rosenhack, "America's Radical Right: Politics and Ideology," in David Apter (ed.), *Ideology and Discontent* (New York: The Free Press, 1964), p. 281; and Sheilah R. Koeppen, "The Radical Right and the Politics of Consensus," in Robert A. Schoenberger (ed.), *The American Right Wing* (New York: Holt, Rinehart & Winston, 1969).

These data suggest two alternative verdicts on the proposition that rapid regional growth produces conservatism. The stronger conclusion is that the proposition is just wrong, for, as Tables 1 and 3 show, San Jose is last or next to last on all three indices of conservative voting, yet it is a strong first among the four metropolitan areas in rate of growth. A second interpretation is that, despite the fifty miles separating them, San Jose is really "part of" San Francisco and its spectacular economic boom has not had the customary political consequences on its residents because of the moderating influence of the older metropolis.[17] This line of explanation requires an important modification of the flat proposition that rapid growth produces ultraconservative political behavior, by introducing the notion that "older civilizations" can ameliorate the results of this growth. It is difficult to imagine this civilizing effect occurring at the mass level, although its effects would be felt there. If it exists, it must be mediated through elite interaction.

This brings us to another type of economic-expansion explanation for the California cleavage, the argument that economic booms produce leadership groups characteristically lacking some of the qualities of elites in more stable areas. The superior political virtue of old wealth has been a familiar theme in speculative writings on McCarthyism and the radical right.[18] As Lipset puts it:

> Although Northern California has continued to increase in population, its major center, San Francisco, has grown little for many decades. There are many old families in the Bay Area who represent four and five generations of wealth. . . . Wealth in Los Angeles, on the other hand, is almost exclusively *nouveaux riches,* and the well-to-do there possess the attitudes toward politics and economics characteristic of this stratum.[19]
>
> New, rapidly expanding centers of population typically lack a leadership structure accustomed to the responsibilities of running community institutions, reducing community tensions, and supporting the rights of various groups to share in community decisions and authority.[20]

As we will show, what we loosely call "elites" in Southern California are far more conservative than their opposite numbers in the North. We question, however, Lipset's explanation of the causes of this difference.

For one thing, his comparison of San Francisco and Los Angeles is somewhat overstated. San Francisco has stopped gaining population in good measure because the city limits, which are firmly defined by the physical setting, were reached by construction almost two generations ago. The Los Angeles city limits, on the other hand, are not so constrained by geography and have included vast tracts of undeveloped land. Los Angeles first eclipsed San Francisco in population in the 1920 Census. It would seem, therefore, that the present generation of wealthy families there cannot be "almost exclusively *nouveaux riches.*" By the same token, the Bay Area's economy has hardly stagnated in the past half century, and so one would expect to find a good deal of first-generation wealth in San Francisco, since the city has continued to be the business center of the area, as well as the financial headquarters of the West.

The conventional wisdom holds that the old rich are likely to have a variety of characteristics conducive to the formation of moderate political attitudes. The rough-hewn, first-generation captain of industry is said to be followed by descendants who indulge the opportunity to acquire the finest educations and consciences and are moved to noblesse oblige by guilt and fashion. These offspring are likely to turn their productive enterprises over to professional managers who, since they are disinterested experts, view social reform with more equanimity than the possessive, insecure men who actually make fortunes. The more bureaucratic, older corporations hire trained experts, use rational planning techniques, and, accustomed to dealing with the government, do not regard it with hostility. The *nouveaux riches* are the opposite of all this. Having come up from the bottom, they tend to be less educated; having had to scramble hard for their money, they find it difficult to modify their acquisitiveness with social conscience and

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[16] "Right-wing extremism, and presumably Goldwater Republicanism as well, is related to the tension of population growth and lack of community integration" (Lipset, "Beyond the Backlash," *op. cit.,* p. 20.).

[17] Aside from a few people in the extreme northern fringe, residents of Santa Clara County (San Jose) do not commute to work in San Francisco.

[18] See, e.g. Bell, *op. cit., The Radical Right, passim.*

[19] Lipset, "Three Decades," *op. cit.,* pp. 436–437.

[20] Lipset, "Beyond the Backlash," *op. cit.,* p. 20.

TABLE 4. REGIONAL DIFFERENCES IN OPPOSITION TO PROPOSITION 14 AMONG THE BETTER EDUCATED AND LESS EDUCATED, BY PARTY[a]

		Per Cent Voting No:		
		Bay Area	Southern California	Association of Region & Vote (Kendall's Q)
Respondents with 3 or more years of college:	Democrats	79%	57%	.485
	Republicans	44%	18%	.560
Respondents with less than 3 years of college:	Democrats	36%	28%	.176
	Republicans	26%	13%	.396

[a] This table includes only white gentile respondents who had made up their minds on Proposition 14 by late October.

are likely to see any political disagreement as a threat to their status and a prelude to revolution.[21]

The foregoing line of thought, which Lipset uses in explaining elite conservatism in Southern California, is in fact quite irrelevant to the subject. Economic growth there, as in the rest of California, is based on "scientific" industries like electronics and aerospace. Firms in these fields typically have very large proportions of professional employees, lay heavy emphasis on rational calculation in decision making, and deal extensively, if not exclusively, with the federal government. The principals in these enterprises often are scientists and engineers. None of this is to say that such businessmen are more liberal, but only that they are a far cry from the traditional image of the *nouveau riche*.[22]

Much more than Los Angeles, San Jose would seem to be a city whose leaders are not "accustomed to the responsibilities of running community institutions." Yet as we have seen, San Jose's voters were no more conservative than those in the presumably staid remainder of the Bay Area; and on Proposition 14 they were more liberal. This suggests that, at the very least, San Jose's inexperienced "leadership structure" was somehow socialized by its proximity to San Francisco's Brahmins. The more drastic—and more plausible—conclusion is that rapid expansion is not a determinant of conservatism.

While we doubt Lipset's hypothesis about the

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[21] *Ibid.*, pp. 19–20. While these presumed political characteristics of *nouveaux riches* may seem to resemble those often attributed to all upward mobile groups, it may be useful to view them separately. One assumes that a new millionaire may be somewhat different from a member of the middle class with working-class parents. In any event, the familiar contention about upward mobile members of the business and professional classes appears to be unfounded. Data showing that the upward mobile are not more ethnically prejudiced have been available for some time (see, e.g., Bruno Bettleheim, *Dynamics of Prejudice* (New York: Harper & Row, 1950), p. 59); more recently the vast preponderance of published research has cast doubt on the notion that upward mobility produces ethnic prejudice. The most extensive study of this point is James A. Barber, Jr., *Social Mobility and Political Behavior* (Chicago: Rand-McNally, forthcoming). Analyzing data from the University of Michigan Survey Research Center's studies of the 1960, 1956, and 1952 elections, Barber found that upward mobile members of the middle class were, compared to status-stable members of the same class, *less* likely to be Republicans and to have conservative attitudes on various political issues. For data showing that supporters of the radical right are, if anything, *less* upward mobile, see Wolfinger *et al.*, *op. cit.*, p. 278; and Koeppen, *op. cit.* The same seems to be the case with Senator Goldwater's followers. See James McEvoy, III, *Radicals or Conservatives: A Study of the Contemporary American Right* (Chicago: Rand McNally, 1969).

[22] Propositions about elites cannot easily be verified with data from surveys of the general population since there are seldom enough elite respondents. The same is true here, but we can use these data to examine the related proposition that self-employed individuals tend to be more conservative than people who do not work for themselves. Examining Republicans and Democrats separately (and excluding non-whites and Jews), we found that the self-employed were no more likely to vote for Proposition 14. Self-employed Republicans showed a slightly greater preference for Goldwater among all Republican presidential possibilities (30 per cent to 24 per cent).

TABLE 5. VOTING BEHAVIOR OF HIGH-INCOME CITIES IN SOUTHERN CALIFORNIA AND THE BAY AREA

	Per Cent Registered Democrats[a]	Per Cent for Johnson	Per Cent "No" on Prop. 14[b]
Southern California			
Mean for region[c]	20	24	20
Palos Verdes Estates	21	28	25
Rolling Hills Estates	29	29	22
San Marino	11	16	14
Beverly Hills[d]	53	65	50
Bay Area			
Mean for region	27	45	40
Atherton	21	42	42
Hillsborough	21	38	35
Los Altos	32	51	43
Los Altos Hills	32	52	52
Piedmont	24	35	29
Ross	30	50	42
Woodside	30	45	40

[a] Registration figures are for the 1964 general election, computed on a base of all registered voters.

[b] A vote against Proposition 14 was a vote *for* fair housing.

[c] The regional means exclude Beverly Hills.

[d] The population of Beverly Hills is predominantly Jewish; 51 per cent of the city's residents were foreign stock in 1960.

causes of the greater conservatism of elites in Southern California, there is no question at all that they are indeed more conservative. If we define "elites" very loosely as people with more money or education, this assertion is easily demonstrated empirically. Table 4 shows regional differences in opposition to Proposition 14 of respondents with three or more years of college and those with less education, with party identification controlled. The regional differences between the better educated are 22 per cent for the Democrats and 26 per cent for the Republicans. They are far smaller among respondents with less education: eight and thirteen per cent, respectively. The strength of the relationship between region and the Proposition 14 vote for each of the four groups can be measured more precisely by means of Kendall's Q. With this measure a value of 1.0 (or -1.0) describes a perfect relationship.[23] As Table 4 shows, the Q scores for the better-educated respondents are considerably larger than for the rest of the sample. These data show that the North-South split in California is much more pronounced among the better educated. They also show that the split is greater among Republicans than Democrats, a point we will return to later.

We cannot explore this subject further with the available survey data,[24] but a combination of Census data and voting returns gives persuasive support to the proposition that regional differences are far greater among upper status groups. Table 5 compares the voting of the most fashionable suburbs in Southern California and the Bay Area, those cities in the two regions with median family incomes in excess of $10,000. Looking at the table, it is difficult to escape the conclusion that these two sets of cities have very different prevailing political styles. Not only are the southern towns far more conservative, but, as a comparison of Tables 5 and 1 shows, the gap between them and their northern counterparts is enormously greater than the overall difference between the two regions. This aggregate data comparison, like the contrasting survey data, suggests that the elite regional split is most strongly affected by differences among Republicans. Residents of both sets of towns are overwhelmingly Republican: four to one in the

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[23] See Hubert M. Blalock, Jr., *Social Statistics* (New York: McGraw-Hill, 1960), pp. 225–231.

[24] The surveys did not include questions on respondents' income and we found the occupation code unsatisfactory for purposes of the present analysis. There were too few cases to permit valid analysis of primary preferences for Goldwater among the highly educated. There was no discernible tendency for a greater proportion of better-educated respondents in the Bay Area to be Democrats (but see Table 5).

South and almost three to one in the Bay Area. The regional difference in Republican registration is not very great, but the northern towns were twice as liberal on Proposition 14 and in the presidential election. Given the small difference in Democratic registration, it is very likely that most of this represents Republicans forsaking Goldwater and Proposition 14.

III. TOWARD A SATISFACTORY EXPLANATION

Drawing on the preceding analysis of socio-economic variations in the regional political split, we can now point to a line of reasoning and investigation that might be pursued in a more satisfactory explanation of California's two political regions.

First, it needs to be emphasized that from a simple statistical standpoint upper-income (or upper-education) groups contribute disproportionately to the regional cleavage. The regional differences in liberalism-conservatism exist at *all* income levels, but if the mean differences were no greater than the gap between poorer people in the two regions, the subject of this article would have attracted far less attention.[25]

Second, we may ask how these regional differences in political behavior are maintained, putting aside for the moment the question of how they arose historically. A good explanation might take the following general form (and we emphasize that the necessary research has yet to be done): The environment of political communication is more conservatively "colored" in Southern than in Northern California. By and large, newspapers in the South have been more conservative than in the Bay Area.[26] Right-wing organizations are far more active, numerous, and vociferous in the South—although in all areas of the state the percentage of the people engaging in such activity is not large. Given the class distribution of exposure to the mass media and political information-seeking, we would expect richer individuals (and communities) to be disproportionately influenced by these environmental effects. But there would be some environmental impact in poorer communities and, in addition, the conservative inclinations of wealthier Southern Californians would become diffused because political and civic leaders and activists are drawn mostly from such groups and because their views spread to other people through business relationships, community organizations, prestige suggestion in interpersonal relations, letters to the mass media, and political activity and contributions. The reciprocal of this argument would, of course, apply to the Bay Area.

The regional split appears to be smallest in partisan general elections, where the party label is a strong guide to voting choices. In primaries and referenda, where political stimuli are not so mediated by party identification, those influences associated with the regional political cultures are likely to have more impact.

Third, there is the question of the historical origin of the regional differences. One not uncommon argument is that San Francisco has long been a stronghold of labor unions, while Los Angeles, with a far different early economic history, was notoriously an "open shop town" until the Second World War. While this is clearly true, regional political differences among working-class voters are relatively small. As a caution to hasty generalization, we should also point out that the strong San Francisco labor movement was responsible for much of the anti-Oriental agitation in that city and in the state from the latter nineteenth century through the First World War.

A second line of historical explanation points to the different sources of immigrants to the two

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[25] Bay Area cities with 1959 median incomes of less than $6000 were only six percentage points more hostile to Proposition 14 than comparable cities in Southern California, while the parallel difference among cities in the over-$8000 category was 19 per cent.

[26] The *Los Angeles Times* has a position of dominance in Southern California far exceeding that of any newspaper in the Bay Area. The *Times* has become relatively more moderate recently (although it favored both Proposition 14 and Goldwater over Johnson), but until the last few years it followed an ultraconservative line in both news and editorial columns.

Regional differences in newspapers' political orientation are particularly noticeable in the suburban press, as is illustrated by the following passage from a *Newsweek* story on Orange County, described as "a monument of monolithic conservatism":

> Pervasive, rather than powerful, is the word for the county's conservative journalism. R. C. Hoiles' "Freedom Newspapers" . . . reflect the creaky views of their 87-year-old owner. "Tax-supported schools violate the Ten Commandments and the Golden Rule," he says, and then advises that the entire public-school system be handed over to "private enterprise."
>
> But Hoiles' papers, and a few other conservative journals, are all that is available in local journalism. Conservatives have the ball game almost to themselves. Liberals—not to mention moderate Republicans or Democrats—are almost literally nowhere. (*Newsweek*, November 14, 1966, p. 37).

TABLE 6. SOURCES OF NEW ARRIVALS IN THE FOUR METROPOLITAN AREAS—1955–60

	San Francisco–Oakland	San Jose	Los Angeles–Long Beach	San Diego
1960 population	2,783,359	642,315	6,742,696	1,033,011
Total new arrivals, 1955–60[a]	416,647	203,066	1,048,175	326,699
Per cent of new arrivals from southern and border states[b]	19	11	22	25
Per cent of new arrivals from the Midwest[c]	15	12	28	23
Per cent of new arrivals from elsewhere in California	36	56	19	26
Per cent of new arrivals from the Northeast	10	8	16	10

[a] *1960 residents of the given metropolitan area who lived elsewhere in 1955.*

[b] 1960 residents of the given metropolitan area who lived in the southern and border states in 1955. The border region includes Delaware, Maryland, the District of Columbia, West Virginia, Kentucky, Oklahoma, and Missouri.

[c] The largest regional differences in out-of-state migrants are found in the contribution of the Great Lakes states, which is far bigger to Southern California.

Source: 1960 Census.

regions, now and/or at some crucial period in the past. After the polyglot Forty-Niners, Northern California was settled by migrants from the northeast and Europe, while Los Angeles' first sizable influx included many more Midwesterners, presumably more Protestant, agrarian, conservative, and intolerant.[27] Some historians attribute the great Progressive election victory of 1910 to a continued flow of moralistic Midwesterners to Southern California.[28] The same distinctive sources of immigration continued in the period between the wars.[29] The last United States Census to include questions about religious affiliation, taken in 1936, revealed that two-thirds of the church members in the Bay Area were Catholics, compared to 36 per cent in the Los Angeles metropolitan area and 44 per cent in San Diego County. Moreover, Bay Area Protestants were evenly divided between fundamentalist and more liberal denominations, while in Southern California half again as many Protestants belonged to the more fundamentalist churches. In 1964 Catholics comprised 40 per cent of the white California Poll respondents in the Bay Area and 24 per cent in Southern California.[30] However there seems to have been something of a convergence in the *sources* of out-of-state immigrants. Table 6 shows the locations in 1955 of those people who moved to the four major metropolitan areas between 1955 and 1960. By and large the Southland has somewhat more new settlers from *both* the South and Northeast, considerably more from the Midwest, particularly from the Great

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[27] Daniel J. Elazar makes particularly imaginative use of the differential migration theme. See his *American Federalism: A View from the States* (New York: Crowell, 1966), pp. 104–105. He does not, however, explain why these different sources would produce conservatism in the South and liberalism in the North.

[28] See, e.g., Franklin Hichborn, *California Politics—1891–1939,* vol. 2 (Los Angeles: Haynes Foundation, no date), pp. 904–905, 1072, ch. 50. The tendency for yesterday's progressives to become today's radical right has been discussed—perhaps beyond the available evidence—by Richard Hofstadter and other social commentators. Nevertheless, this points to the need for a careful examination of indices of conservative and liberal behavior in California in the past two generations. Some measures that might be considered "liberal," such as the Townsend Plan, fared better in Southern California. *Cf.* Carey McWilliams, *Southern California Country* (New York: Duell, Sloan and Pearce, 1956), chap. 14.

[29] James Q. Wilson, "A Guide to Reagan Country: The Political Culture of Southern California," *Commentary* (May, 1967), p. 38.

[30] Since many nominal Protestants, unlike Catholics, are not formally members of a church, measures of religious affiliation which rely on church membership inevitably inflate the proportion of Catholics in the population. Therefore the survey data are a more accurate measure of the absolute number of Catholics and Protestants.

Lakes states, and far fewer migrants from other parts of California itself. Half the residents of both northern metropolitan areas were born in California, compared to a third for each of the two southern areas.

Granted that there are more Catholics and native Californians in the Bay Area, and more fundamentalists, Southerners, and Midwesterners in the Southland, what are the political consequences? Using Census data and voting returns, we correlated voting on Proposition 14 and the percentage of the population born in California for all incorporated cities in the state, and found no relationship between these two variables. We have no data on individual respondents' original places of residence, which would let us see if this variable is related to contemporary political behavior, but we can compare our survey respondents' political preferences and religious affiliations. The data, while not adequate for anything but suggestive purposes, do indicate that part of the regional political difference *may* be a result of different backgrounds of residents of the two areas. As we would expect, California Catholics are more likely to be Democrats than are Protestants.[31] In the Bay Area 51 per cent of white Protestants are Democrats, compared to 78 per cent of the white Catholics. In Southern California the corresponding figures are 44 per cent and 74 per cent.[32] When we examined these respondents' attitudes toward the Rumford Fair Housing Act, controlled for party identification, religion, and region, the number of Republicans dwindled so as to preclude meaningful analysis. The number of Democratic cases in each cell is almost as small, but the data indicate that both religion and region were associated with support of the Rumford Act. In Southern California 26 per cent of the Protestants and 42 per cent of the Catholics favored it; in the Bay Area, 39 per cent of the Protestants and 49 per cent of the Catholics.[33]

In short, there are proportionately more Catholics in the Bay Area than in Southern California, and the more Democratic, liberal inclinations of the former section may be accounted for to some extent by this demographic difference. It seems likely to us that the regional religious variation reflects a more general difference in the background of settlers in the two sections: more cosmopolitan and urban in the North, more rural in Los Angeles. Yet, tempting as this conclusion is, we mention it with considerable trepidation. It is based on a scanty number of cases and involves only a single, possibly misleading index of liberalism. The Catholic Church exerted great efforts in behalf of the fair housing cause in the 1964 election; we have no way of knowing how much of the Catholic voters' support of the Rumford Act was due to this pressure and consequently cannot be interpreted as a sign of more general liberal attitudes. Indeed, in the country as a whole Catholics do not seem to be more favorably inclined toward fair housing. A Gallup Poll taken in 1967 showed no differences between Catholics and Protestants in their opinions of a federal fair housing bill (both groups were decisively against it);[34] if the southern respondents were removed from this sample, doubtless the result would be to make the remaining Protestant respondents considerably more liberal than the Catholics. Finally, it should be noted that the regional difference in the views of white Protestant Democrats was considerable: 26 per cent in Southern California and 39 per cent in the Bay Area favored the Rumford Act. Hence religion (and whatever regional differences in population characteristics it represents) is not the sole explanation of the regional split we have been attempting to analyze.

In addition to the propositions about historical origins, there are what might be called "intermediate historical propositions" which may prove to fit into whatever causal explanations are eventually developed. Thus once a region gains a reputation for a particular life style—in politics or whatever—its cultural and political climate may be reinforced by selective migration. Business and professional men have more freedom to pick their own homes than most other people, and "the kind of people" who find Southern California more congenial may be more conservative than those who, also able to choose their home, like the Bay Area better.

While we cannot provide firm affirmative propositions explaining the roots of the striking regional differences in California politics, we can be fairly sure about three statements: (1)

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[31] For data on Catholics' Democratic proclivities see Bernard R. Berelson, *et al., Voting* (Chicago: University of Chicago Press, 1954), pp. 61–71; Angus Campbell, *et al., The Voter Decides* (Evanston: Row, Peterson & Co., 1954), p. 71; Angus Campbell, *et al., The American Voter* (New York: Wiley, 1960), ch. 12; and Scott Greer, "Catholic Voters and the Democratic Party," *Public Opinion Quarterly,* 25 (Winter, 1961), 611–625.

[32] These percentages are computed on a base of respondents who identified with one major party or the other.

[33] These percentages are computed on a base of respondents who had an opinion on the Rumford Act.

[34] Helen Erskine, "The Polls: Negro Housing," *Public Opinion Quarterly,* 31 (Fall, 1967), p. 491.

Southern California's conservative orientation is not a consequence of its rapid economic development and population growth. (2) The differences in conservatism between Southern and Northern California are at least partially independent of gross differences in the present population characteristics of the two regions. (3) These political differences are determined in good measure by the different political preferences of leaders and activists in the two regions.

These findings should reassure those who fear that illiberal political movements are the consequences of vertical and horizontal social mobility, economic growth, and suburbanization—trends which are likely to be with us for some time to come.

ALIENATION AND POLITICAL BEHAVIOR*

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I. INTRODUCTION: ALIENATION

Alienation is both one of the most popular and vague concepts used by contemporary social scientists. Scholars often cite Robert Nisbet's statement that alienation is basically a perspective.[1] The current age is said to be one of alienation, or writers declare alienation to be the fundamental interpretive concept for explaining deviant behavior. One author has even gone so far as to say that definition is unnecessary because we can all feel what "it" is in our very bones. Indeed, if we don't understand it intuitively we are alienated by definition.[2]

Recently, there have been a series of attempts to clarify the meaning of the term. Daniel Bell, commenting on the uses of the concept alienation in the works of Marx, distinguishes *estrangement* ("a socio-psychological condition") from *reification* ("a philosophical category with psychological overtones").[3] For research purposes, the fundamental difference between these meanings lies in the criteria which are applied in determining whether an individual is alienated. The existence of estrangement is determined by investigating the attitudes of individuals; reification is measured against "objective" standards about the quality of human life established by the investigator.

The reification (objective) tradition has many strong exponents.[4] It offers a potentially powerful concept to an analyst wishing to evaluate the human condition in terms of explicitly stated criteria of what man *ought* to be in his social and personal relationships. Most of the contemporary scholarly work, however, is concerned with estrangement, and my own interest also lies in the individual's perception of the situation he faces.

Merely limiting ourselves to determining the existence or degree of alienation by gauging the attitudes of individuals is but a small step forward in clarifying what we mean by the term. Fortunately, two contemporary scholars have laid a solid foundation for a more adequate conceptualization. Melvin Seeman's "On the Meaning of Alienation" systematically isolates five "basic ways in which the concept of alienation has been used" in the literature.[5] He suggests an effort to explore the interrelationships between operational measures of these variants and also the causes and consequences of them.[6] Kenneth Keniston, in a very concise appendix to his study of alienated youth, suggests that[7]

> . . . while the concept of alienation in every variation suggests the loss or absence of a previous or desirable relationship, it requires further specification in at least four respects:

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[1] Robert A. Nisbet, *Community and Power* (New York: Oxford University Press, 1962), p. 15.

[2] Gerald Sykes (ed.), *Alienation: The Cultural Climate of Our Time* (New York: George Braziller, 1964), Volume 1, p. xiii.

[3] Daniel Bell, "The Rediscovery of Alienation: Some Notes Along the Quest for the Historical Marx," *Journal of Philosophy*, 56 (1959), pp. 933–934.

[4] See, for example, Erich Fromm's introduction to Erich Fromm (ed.), *Marx's Concept of Man* (New York: F. Ungar Publishing Co., 1961); and C. Wright Mills, *White Collar* (New York, Oxford University Press, 1951).

[5] Melvin Seeman, "On the Meaning of Alienation," *American Sociological Review*, 24 (1959), p. 783. The five "variants" (as Seeman calls them) are: 1) Powerlessness; 2) Meaninglessness; 3) Normlessness; 4) Isolation; and 5) Self-Estrangement.

[6] Several of Seeman's students and others inspired by him have been particularly interested in empirical investigations of the dimensionality of these variants. See, in particular, Dwight G. Dean, "Alienation: It's Meaning and Measurement," *American Sociological Review*, 26 (1961), 753–758; and Arthur G. Neal and Salomon Rettig, "On the Multidimensionality of Alienation," *American Sociological Review*, 32 (1967), 54-64.

[7] Kenneth Keniston, *The Uncommitted: Alienated Youth in American Society* (New York: Harcourt, Brace and World, Inc., 1965), p. 454.

1. *Focus:* Alienated from what?
2. *Replacement:* What replaces the old relationship? (This might more crisply be referred to as the *form* of the alienation.)
3. *Mode:* How is the alienation manifest?
4. *Agent:* What is the agent (cause) of the alienation?

It is evident, as Keniston points out, that these four questions "provide a basis for a virtually limitless number of varieties of alienation."[8] A person must naturally wonder if it might not simply be better to discard the term. This solution is quite appealing, but Keniston speaks of the certainty in this case "that the same problems of definition would crop up again and again with some cognate term like estrangement, disaffection, or detachment."[9] An even more important reason for at least temporarily retaining the term, I think, is that it has been employed in a great number of widely used theories and one task of scholars at this point should be to clarify when and how various forms of alienation do serve as important independent or intervening variables and whether these attitudes are meaningfully structured in people's minds. The Keniston scheme is valuable for empirical and theoretical purposes because it stimulates definitional and measurement refinements (i.e., looking at alienation in terms of form [subject] and focus [object]), and emphasizes the place of the various types of alienation in a chain of sources and consequences.

In this paper I have taken operational measures of two commonly accepted forms of alienation (trust and efficacy),[10] in each case provided two foci for each form, and examined the relationship between individuals' scores on these indicators and their political behavior. I am trying to discover the value of measuring alienation in this way—i.e., do the various forms of alienation lead to different types of behavior and does the focus of the measurement make a difference?

While a test of the utility of Keniston's scheme in empirical research is valuable in itself, there is a broader set of questions raised in the existing literature on alienation and political behavior. Basically, these concern the conditions which stimulate the political mobilization of the alienated. Therefore, before presenting the data, I will review the relevant literature, giving special attention to discussions of the political settings in which attitudes of alienation are important determinants of behavior and to contradictions in research findings which appear to result from inadequate consideration of the forms and foci of alienation. This review emphasizes the literature on American political behavior since it is the subject matter of the most systematic research and also the setting for the data employed here. I begin with authors who do not use direct measures of both alienation and behavior in establishing relationships and then consider those who do.

II. SOME VIEWS IN THE LITERATURE

One prominent interpretation is that the alienated (the term is usually very broadly defined) are quiescent under ordinary circumstances, but subject to mobilization into mass movements (or at least support of extremist, demagogic and/or authoritarian political leaders) when the material or psychological circumstances are proper and the right leader presents himself.[11] Empirical studies done in the United States on support for extremist leaders and groups do not present a very clear picture. Thus, while Nelson Polsby found that Senator McCarthy gathered the bulk of his support in areas "where the Republican Party was strongest"[12] and that his reputed influence on elections outside of Wisconsin had no basis in fact, Michael Rogin's study of George Wallace's 1964 Democratic presidential primary campaign in Wisconsin demonstrated that Republican crossover voters (particularly in middle- and upper-class districts) gave him substantial support.[13] The implication of the first study is that McCarthy's impact on the behavior of voters was negligible, but the second indicates that many Republicans may compose a natural reservoir of support for classic American demagogues.[14] Some substantiation of the latter point is found in the Wolfinger, *et al.*, study of Christian Anti-communist Crusaders where the most salient characteristic of the respondents studied was that they were Republicans.[15]

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[8] *Ibid.*, p. 454.

[9] *Ibid.*, p. 452.

[10] For an extended discussion of the forms of alienation commonly used in the literature and a critique of Seeman's article, see Joel D. Aberbach, *Alienation and Race* (unpublished Ph.D. Dissertation, Yale University, 1967), especially pp. 7–12 and 71–74.

[11] See, for the best known example, William Kornhauser, *The Politics of Mass Society* (Glencoe: Free Press, 1959). For Kornhauser, alienation is described rather indiscriminately in several forms. He employs operational measures of political inefficacy, social distrust and low ego strength in his secondary analyses and alludes to expressions of hostility and resentment against established institutions in the text. (See, among others, pp. 109–112, 148 and 166.) Economic dislocation, social shock caused by events like defeat in war and/or social confusion accompanied by demogogic attack on the existing political system can arouse the public susceptible to mass appeals. The mass man (who suffers from "a lack of proximate attachments") lacks internalized standards and is, therefore, politically volatile. When there are no clear signals available his anxiety is mixed with political apathy, but spasmodic "flights into activity" are also characteristic. Precise predictions about the behavioral and psychological results of alienation are not made by the theory except at the extremes of a continuum which runs from no signals (resulting in apathy) to appeals in time of acute societal distress (yielding active mass movements).

A more general principle may do something to reconcile any surface contradictions in these studies: In clearly structured general elections, standard reference symbols like party and group govern the direction of voting behavior; but in situations where these symbols provide no clear guide (primaries, nonpartisan elections, decisions to join movements of various types), other factors can assume great importance. In the Rogin (Wallace) case, many Republicans were people prone to "express their general resentment against outside interference in their lives—from government as well as Negroes"[16] and gave this feeling free reign (along with certain working-class Democrats) in a primary where an issue appropriate to this sentiment was raised, while in the Polsby (McCarthy) case voting was more clearly on party lines and motivations based in resentment were more obscure.

Support for this interpretation can be found in the literature on the impact of personality on politics ably summarized by Greenstein.[17] Drawing on the writings of Lane, Shils, Christie and Jahoda, Levinson and others in the field, Greenstein stresses that an unambiguous environment reduces the opportunities for personal variations in behavior. Where the situation is clearly structured—for example, an attractive reference group takes a clear and definite stand, or strong sanctions are attached to certain behavior alternatives—the scope for personality to affect behavior is severely limited. However, where salient reference groups have conflicting points of view, or "current situations . . . are in conflict with previous experience," etc., the individual's clues are not clear and personality factors can play a key role.[18]

Writers employing psychological measures of alienation present evidence which appears to confirm this general principle. For example, detailed investigations of referendum voting consistently emphasize the negativism of the politically alienated "lead [ing] to an attitude on a given issue which represents a protest against the existing structure in the community."[19] The alienated, therefore, tend to express their resentment in negative votes on bond issues and the like. This could, of course, also explain the behavior of certain Republicans in the Wallace primary.

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[12] Nelson W. Polsby, "Toward an Explanation of McCarthyism" in Nelson W. Polsby, Robert A. Dentler and Paul A. Smith (eds.), *Politics and Social Life* (Boston: Houghton Mifflin Company, 1963), p. 816.

[13] Michael Rogin, "Wallace and the Middle Class: The White Backlash in Wisconsin," *Public Opinion Quarterly,* 30 (1966), 98–109. On page 100 he says that "In Wisconsin, a state where Republicans can vote in the Democratic primary, Wallace's vote closely resembled the normal Republican vote." This analysis involves ecological correlations and thus all of the risks which go with it but Polsby's basic technique is similar.

[14] *Ibid.,* p. 106. Rogin, it should be noted, points to the fact that there were some differences in the support attracted by Wallace and McCarthy. Wallace was stronger in working-class areas, while McCarthy did much better in rural areas. But he emphasized that "each was strong among conservative Republicans" (p. 105). This is a deduction supported by some reasonable evidence, but Rogin discounts the unpopularity of Governor Reynolds who stood in for Johnson in the primary and the possibility that Republicans crossed over to vote for Wallace in order to embarrass the Democrats. These data, of course, cannot be used to make a definitive conclusion in this area.

[15] Raymond E. Wolfinger, Barbara Kaye Wolfinger, Kenneth Prewitt, and Sheilah Rosenhack, "America's Radical Right: Politics and Ideology," pp. 262–293, in David E. Apter (ed.), *Ideology and Discontent* (New York: The Free Press of Glencoe, 1964). See especially, p. 288. The subjects in this study felt quite efficacious politically (and, therefore not alienated in the powerlessness sense), were fundamentalist Goldwater supporters and "old-fashioned individualists."

[16] Rogin, *op. cit.,* p. 106.

[17] Fred I. Greenstein, "The Impact of Personality on Politics: An Attempt to Clear Away Underbrush," this REVIEW, 61 (1967), 629–642. Personality is not defined by Greenstein because psychologists themselves have not come close to an agreed-upon meaning of the term and, more importantly, because he is really talking about "psychological factors" (p. 630) which is a very broad category indeed. This broad focus is forced upon him by the content of the literature being reviewed as much as by any personal preference.

[18] Robert E. Lane, *Political Life* (Glencoe: Free Press, 1959), p. 99.

A more detailed look at these studies is in order because of the nature of the measures employed and the different arenas (foci) considered. In some cases combined (composite) measures of political distrust (cynicism) and powerlessness are used.[20] The focus of the measurement items in these cases is clearly the local government and the dependent variable is vote in a local referendum. Composites are useful devices for either obtaining a more homogenous and precise measurement of a variable or for obtaining a better device for predicting another variable.[21] However, forming them on an *ad hoc* basis (i.e., without either of the above purposes clearly in mind) may disguise the relationship of some of the components with outside variables or otherwise distort the results.[22]

My argument is not that it is illegitimate to base an operational definition of political alienation on an assertion that alienation "really" means, for example, a state in which an individual is powerless and distrusts those who govern, but that using an *ad hoc* composite to measure political alienation is self-defeating and leaves unanswered a series of nagging questions. Just how are the indicators related? Which part of the composite index is causing the relationships described or, better, how does each indicator relate to the various dependent variables? Perhaps they operate in the same way, maybe only one plays a significant role, or perhaps there are interesting interactions between the indicators which are significant theoretically and empirically.[23] The fundamental reason for defining alienation as a combination of distrust and powerlessness is the expectation that the joint occurrence of the two attitudes produces a unique behavioral tendency[24] and this should be established rather than, as in the case of these authors, assumed.

Other researchers in this area have used powerlessness measures alone in their work or have made some incomplete tests on the independent effects of powerlessness and distrust. Horton and Thompson, for example, emphasize that "referendums may serve as institutional outlets for protest . . . on the part of the powerless and ordinarily apathetic members of the community."[25] They operationally define political alienation as a feeling of powerlessness and an identification of an appropriate power center as a controlling agent.[26] They not only find the expected relationship between the composite index of powerlessness and negative voting, but they present some evidence that the feeling of powerlessness by itself has an independent effect on voting behavior in this instance.[27] They do have a direct measure of the distrust of local government, but it is unfortunately not discussed in terms of political behavior.

William Gamson's work on attitudes toward water fluoridation proposals is also germane here. He cites Coleman's suggestion that this issue gives citizens an opportunity to vent their frustrations against local administrators who introduce the idea.[28] Gamson attempted to gauge ideological differences between respondents in his Cambridge, Massachusetts sample, but found that "overt differences in ideology concerning individual rights and government intervention have little to do with the average voter's position on fluoridation in this Cambridge precinct."[29] However, alienation measures or, better, some alienation measures were useful as predictors. Indicators of powerlessness and meaninglessness did successfully discriminate opponents of fluoridation from proponents,[30] but measures of distrust did not. Unfortunately, the measures of powerlessness were political and those of distrust were not, but we can see that distrust is not necessarily related to issue negativism.

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[19] Wayne E. Thompson and John E. Horton, "Political Alienation as a Force in Political Action," *Social Forces,* 38 (1960), p. 195.

[20] *Ibid.;* and Edward L. McDill and Jeanne Claire Ridley, "Status, Anomia, Political Alienation and Political Participation," *American Journal of Sociology,* 68 (1962), 205–214. No test of unidimensionality is reported.

[21] Edwin E. Ghiselli, *The Theory of Psychological Measurement* (New York: McGraw-Hill, 1964), p. 184. When the analyst wishes to make his measurement of a variable more homogenous and precise he chooses elements which have relatively high intercorrelations. One does not greatly increase the correlation between a composite variable and an outside variable if the components added to form the composite are highly correlated. However, when a composite is developed for purposes of prediction, the components will probably be correlated at a low level with each other and the addition of each element will greatly increase the level of correlation with the outside variable. The goal in this case, then, is a composite formed of relatively independent components each highly correlated with the outside variable we wish to predict.

[22] See Richard F. Curtis and Elton P. Jackson, "Multiple Indicators in Survey Research," *American Journal of Sociology,* 68 (1962), 195–204.

[23] This argument is made in detail in Aberbach, *op. cit.,* Ch. 2. I am currently preparing an article which reviews the literature on alienation in these terms.

[24] Clarence N. Stone makes a simular point in "Local Referendums: An Alternative to the Alienated-Voter Model," *Public Opinion Quarterly,* 29 (1962), 213–222.

[25] John E. Horton and Wayne E. Thompson, "Powerlessness and Political Negativism: A Study of Defeated Local Referendums," *American Journal of Sociology,* 67 (1962), p. 485.

[26] There is an implied, but not explicit, element of distrust in the second element of the definition.

[27] Horton and Thompson, *op. cit.,* p. 490, Table 1.

Clearly, on these local issues, a perceived sense of powerlessness does lead to negative voting or to opposition to certain controversial programs. However, it is not certain just what effect distrust of local government exerts and how it interacts with feelings of powerlessness.

It is also still not clear that attitudes of alienation (of any form) play a role in voting in all types of referendums. One author has suggested that alienation is important in determining direction of vote only in referendums where "vague emotion-laden symbols are employed"[31] and that the alienated may turn out in large numbers in other types of referendums and vote like other citizens. There is, obviously, still a great need for research on the whole question of turnout, alienation and vote in local referendums and, for that matter, in local partisan elections.

If research and knowledge on the relationship between alienation and vote are somewhat confused and definitely incomplete on local elections, they are virtually non-existent on national elections. Templeton found alienation, in this case measured by Srole's Anomia scale,[32] unrelated to respondents' party identification or voting behavior in 1956 and 1960.[33] However, following Levin and Eden,[34] he finds some inferential support for the notion that the "quality" of the vote cast by an alienated voter may be different, i.e., that he tends to vote negatively and is most inspired when issues are perceived in terms of good and evil. Since, under most circumstances, "both major parties present themselves as 'responsible' national voices and both are in fact committed to the established political system . . . neither provides the citizen with the opportunity to validate his personal rejection of the political system."[35] As a result of this, says Templeton, the alienated citizen tends to withdraw from participation in the national political process. He manifests this through inconsistent voting (vacillating between the parties), and low political knowledge and interest,[36] although no evidence is presented of unusual nonparticipation in voting *per se*. According to this view, the whole process of national politics as usually practiced in this country serves to insulate us from a direct impact of the alienated feelings of part of the electorate.

Reported data on political efficacy are replete with findings of nonparticipation, low interest, etc., on the part of the inefficacious,[37] but there is no evidence that they are "protest voters" in national elections. Again, this may well be a function of the choices and symbols presented in these contests where party and group identifications provide such strong guides to behavior.

An analysis by Donald E. Stokes of the public's "basic evaluative orientations toward the national government"[38] (political trust) based on the SRC 1958 election study failed to turn up any very exciting positive information. Political trust was virtually unrelated to direction of party identification (and vote), but associated with rejection of the party system as indicated by failure to identify oneself along the partisan spectrum.[39] There was a relationship with sense of political efficacy (which Stokes sees as lying behind the individual's attitude toward government) and a slight tendency for poorly educated respondents scoring low on political trust to participate less frequently than others. Stokes feels that those who are low in trust have no articulate commitment to a fundamental change in the political order or to extremist movements or leaders, but that their negative orientation "must . . . be seen as latent support that might be tapped by parties or factions or individual political leaders whose stance is hostile to the prevailing order."[40] The basic and unresolved issue is still when and if this latent support can be tapped in elections given the role of the institutions of our political system in dampening the behavioral effects of attitudes of alienation.

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[28] William Gamson, "The Fluoridation Dialogue," *Public Opinion Quarterly*, 35 (1961), p. 533; citing James Coleman, *Community Conflict* (Glencoe: Free Press, 1957).

[29] Gamson, *op. cit.*, p. 531.

[30] *Ibid.*, p. 535. As in the other studies, these relationships hold with social status controlled.

[31] Stone, *op. cit.*, p. 222. He includes school bonds in this category along with fluoridation and charter elections. More "concrete" issues which Stone hypothesizes draw large turnouts, but do not activate feelings of alienation, include such things as water supply, fire protection, etc. This is the familiar distinction between style and position issues.

[32] Fredric Templeton, "Alienation and Political Participation," *Public Opinion Quarterly*, 30 (1966), p. 252. In light of our discussion of alienation in terms of form and focus it should be noted that Templeton believes "it . . . unlikely that the present findings would be appreciably modified by using one of the other currently available measures of alienation."

[33] *Ibid.*, pp. 254–255.

[34] Murray B. Levin and Murray Eden, "Political Strategy for the Alienated Voter," *Public Opinion Quarterly*, 26 (1962), 47–63, citel in Templeton, *op. cit.*, p. 255.

[35] *Ibid.*, p. 256. Note that Templeton believed the 1964 election might constitute a "partial exception to this generalization."

[36] *Ibid.*, p. 256.

[37] See, for example, Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, 1960), pp. 103–105.

[38] Donald E. Stokes, "Popular Evaluations of Government: An Empirical Assessment" in Harlan Cleveland and Harold D. Lasswell (eds.), *Ethics and Bigness: Scientific, Academic, Religious,*

A graphic example of this can be seen in investigations using the murky variable, authoritarianism. These yield some evidence that this attitude, which contains a strong element of hostility, has a much weaker impact on behavior in general elections than its effect on candidate preferences might indicate. While Lane, using SRC data, found a small relationship between authoritarianism and both Republican party identification and vote for Eisenhower in 1952,[41] Milton's study of the presidential preferences of University of Tennessee students in 1952 demonstrated that neither Eisenhower nor Stevenson received strong support from authoritarians, although Eisenhower was clearly preferred to Stevenson. With a more varied slate of nominees presented to them, authoritarian students opted heavily for Taft and MacArthur, two candidates whose names were not on the official ballot.[42] Two basic points in the Milton study should be noted: 1) There was a strong correlation "between the authoritarianism attributed to aspirants for candidacy and the F-scale scores of students preferring each man"[43] in this preference test without partisan structure; and 2) The actual nominees were those least attractive to authoritarians, although one candidate (Eisenhower) was preferred.

Given the above, to provide some satisfactory test of the effects of attitudes of alienation on political behavior in national elections we need an appropriate set of stimuli. For science, the 1964 election was a blessing.

III. SETTING AND PROCEDURES

In 1964 the Republicans nominated a candidate widely labelled as an "extremist",[44] a man who offered people "a choice, not an echo," and whose covert appeal to racism (in addition to an overt appeal to the old American values) was at least given credence in the mass media if it was not based in his own intentions. This represented a major opportunity for investigating the relationship between attitudes of alienation and direction of voting behavior in a situation conducive to the joint variation of the two in a national election. The 1964 election study of the Survey Research Center at the University of Michigan provides a vehicle for such an investigation.

Fortunately, this study contained four separate measures of alienation as well as rich information on political attitudes and voting behavior. There were indicators of political trust (a slightly modified version of the Stokes scale) and interpersonal trust (trust in people),[45] po-

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Political and Military (New York: Harper and Brothers, 1962), p. 64.

[39] *Ibid.*, p. 69. Stokes is referring to the small number of people who fall into the SRC a-political category and not to those who classify themselves as independents.

[40] *Ibid.*, p. 72.

[41] Robert E. Lane, "Political Personality and Electoral Choice," this REVIEW, 49 (1955). 173–190. See Table IV on p. 181 and the conclusion, p. 190.

[42] O. Milton, "Presidential Choice and Partisan Performance on a Scale of Authoritarianism," *The American Psychologist*, 7 (1952), 597–598. The reference is from Richard Christie, "Authoritarianism Re-examined," pp. 123–197 in Richard Christie and Marie Jahoda (eds.), *Studies in the Scope and Method of "The Authoritarian Personality"* (Glencoe: The Free Press, 1954).

[43] *Ibid.*, p. 146.

[44] This was a part of the rhetoric of the campaign in the primaries as well as the national election, but was also accepted by less directly partisan individuals. See, for example, Robert A. Dahl, "The American Oppositions," pp. 65–68 in Robert A. Dahl (ed.), *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966), where he discusses the response of the "Alienated Right" (p. 66) to both Joe McCarthy and Barry Goldwater. Both, he says, wished to "shake off the shackles of two-party pragmatic, compromising politics and offer a radical and uncompromising alternative."

[45] Morris Rosenberg, "Misanthropy and Political Ideology," *American Sociological Review*, 21 (1956), 690–695, developed the basic scale.

TABLE 1: ALIENATION ITEMS

A. Interpersonal Trust (Trust in People)

1. Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?
2. Would you say that most of the time people try to be helpful or that they are mostly just looking out for themselves?
3. Do you think most people would try to take advantage of you if they got a chance or would they try to be fair?

B. Political Trust (Prefaced by the following statement: Now I'd like to talk about some of the different ideas people have about the government in Washington and see how you feel about them. These opinions don't refer to Democrats or Republicans in particular but just to the government in general. For example:)

1. Do you think that quite a few of the people running the government are a little crooked, not very many are, or do you think hardly any of them are crooked at all?
2. Do you think that people in the government waste a lot of money we pay in taxes, waste some of it, or don't waste very much of it?
3. How much of the time do you think you can trust the government in Washington to do what is right—just about always, most of the time, or only some of the time?
4. Do you feel that almost all of the people running the government are smart people who usually know what they are doing, or do you think that quite a few of them don't seem to know what they are doing?
5. Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?

C. Personal Efficacy (Personal Effectiveness)

1. Have you usually felt pretty sure your life would work out the way you want it to, or have there been times when you haven't been very sure about it?
2. Do you feel that you are the kind of person who gets his share of bad luck or do you feel that you have mostly good luck?
3. When you make plans ahead do you usually get to carry out things the way you expected, or do things usually come up to make you change your plans?

D. Political Efficacy (Agree—Disagree Statements)

1. Voting is the only way that people like me can have a say about how the government runs things.

TABLE 1 (*continued*)

2. People like me don't have any say about what the government does.
3. Sometimes politics and government seem so complicated that a person like me can't really understand what's going on.
4. I don't think public officials care much what people like me think.

litical efficacy and personal efficacy (personal effectiveness).[46] In other words, we have measures of two forms (subjects) of alienation and two foci (objects) for each form. As Table 1 indicates, the items in the trust-in-people and personal-efficacy measures refer to the self and the environment in a very general way; they ask about "most people," "luck," and "life" working out, etc. The political trust items, on the other hand, refer to the government in Washington very specifically and the political efficacy questions have a manifest political content.[47] A factor-analytic test shows that these items, as predicted, fall empirically on distinct psychological dimensions, although the factors are positively correlated.[48] Available evidence, therefore,

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[46] Angus Campbell, *et al., op. cit.*, pp. 516–517, developed this measure as an "agree-disagree" scale. It has since been modified.

[47] It would be better to have political efficacy questions which focus on the federal government also. See Edgar Litt, "Political Cynicism and Political Futility," *The Journal of Politics*, 25 (1963), 312–323, for an example of the potential importance of this. I am currently conducting a study designed, in part, to check the relevance of focus in measuring efficacy.

[48] Principal component factors with eigen-values (amount of variance explained) greater than one were rotated using both orthogonal and oblique criteria. The four factors extracted explained about 65 percent of the variance of all variables. The clarity of structure was marked, with the items for each measure showing strong loadings on separate factors. An oblique factor analysis using the biquartimin solution recommended by Harmon (see Harry Harmon, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1960), pp. 261–337) yielded the following correlations between the primary factors:

	Trust in People	Political Trust	Political Efficacy	Personal Efficacy
Trust in People	—	.16	.11	.13
Political Trust		—	.07	.12
Political Efficacy			—	.05
Personal Efficacy				—

See Aberbach, *op. cit.*, pp. 75–101 for a full dis-

TABLE 2. T_c CORRELATIONS BETWEEN ATTITUDES OF ALIENATION AND VOTE, 1964

Attitude	Interpersonal Trust	Political Trust	Personal Efficacy	Political Efficacy
Vote* (1 = Johnson) (0 = Goldwater)	−.01	.39	−.03	−.05

* A positive coefficient indicates that the higher the person's score on the index (high score equals positive attitude or allegiance), the more likely he was to vote for the Democrat.

points to the fact that thinking about alienation in terms of form and focus should be useful empirically as well as conceptually.

I have constructed separate standardized indices for each factor in the form of T-scores (i.e., with a mean of 50 and a standard deviation of 10) and, for purposes of this analysis, consolidated the scales so that each value represents a standard deviation unit. These scales are then related to vote in the 1964 election and, where results indicate the utility of further exploration, are correlated with additional variables under controlled conditions. Sometimes comparisons are made to relationships in other elections. The goal is always to illuminate, in as much depth as a secondary analysis permits, the conditions under which these attitudes of alienation are relevant to behavior.

The analysis is limited to white respondents because of one stark fact: there is simply no meaningful variation in the direction of Negroes' votes in 1964. This comes as no surprise in light of the issues involved, but illustrates the general principle that when a highly salient reference group's interests are threatened in a political contest, its members behave in a uniform manner if a clear behavioral option is open.[49]

IV. FINDINGS

A look at the relationship in Table 2 between the different attitudes of alienation measured here and the direction of vote in 1964 reveals rather clearly that *only* political trust was a viable zero-order predictor of vote in this election.[50] Table 3 illustrates the true strength of

TABLE 3. POLITICAL TRUST AND VOTE, 1964

Political Trust*		0	1	2	3	4	5
Vote	Goldwater	62.7%	52.3%	36.5%	29.9%	20.9%	13.5%
	Johnson	37.3%	47.7%	63.5%	70.1%	79.1%	86.5%
	N =	185	149	104	197	215	156

* In T-score terms (i.e., a mean of 50 and standard deviation of 10):
0 = Below 40
1 = 40–45
2 = 46–50
3 = 51–55
4 = 56–60
5 = Above 60

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cussion. These findings will be reported in detail in an article I am now writing. Similar findings are reported in Arthur G. Neal and Salomon Rettig, "On the Multidimensionality of Alienation," *American Sociological Review,* 32 (1967), 54–64.

[49] Negro bloc-voting is not uncommon, even in less dire situations. John C. Legget, for example, studied the 1958 gubernatorial election in Michigan using a sample of 375 Detroit blue-collar workers. He found that class consciousness and union militance had a strong influence on white workers' voting behavior (the more militant voted for the reform candidate), but that Negroes voted for the reform candidate regardless of militance or union membership. See John C. Legget, "Working Class Consciousness, Race and Political Choice," *American Journal of Sociology,* 69 (1963), 171–177.

[50] The ordinal correlation used here is Kendall's tau_c (T_c). It ranges between 0 and 1, but reaches unity when all cells are empty except those in the diagonal. Since this rarely happens, T_c seldom approaches unity and is a very conservative (although sensitive) measure of association. See Maurice G. Kendall, *Rank Correlation Methods* (London: C. Griffin, 1955), Chs. 1 and 3; and Hubert

this relationship. The others are substantively trivial. Even interpersonal trust, which is seen by many as the fundamental underpinning of political trust,[51] did not successfully predict the candidate chosen by the electorate on this occasion. Clearly, both the form and focus of alienation are important factors to consider in any study.

An immediate question arises as to whether political trust, as measured here, is really just a proximal partisan attitude of the type used so successfully by the authors of *The American Voter* in predicting vote.[52] This is not the case. Campbell *et al.* found party identification very strongly related to each of the partisan attitudes they described,[53] but in 1964 political trust was only mildly related to party identification ($T_c =$ .15, i.e., Republicans were less trusting than Democrats). The strength of the relationship between political trust and vote ($T_c =$ .39) goes well beyond what one might expect from the measure of association given for party identification.

It is useful to compare the relationships between party identification, political trust, and vote in 1958 and 1964. It would probably be better to make a direct comparison of two Presidential elections,[54] but when I did this analysis these were the only election studies available where the relevant data were gathered, and there is reasonable evidence that the 1958 data are not atypical on the relationship of political trust to both party identification and vote. In 1958, political trust was negatively correlated with Democratic party identification ($T_c =$ −.06, i.e., the Democrats were slightly more distrustful) and Democratic vote for Congress ($T_c =$ −.08). There is, then, some evidence that a mild partisan element is captured by the measure of political trust since the followers of the party *out* of power tend to be less trusting,[55] but the behavioral relevance of this attitude in 1964 went well beyond this. The special significance of the 1964 election in this regard is manifested in the correlation between political trust (measured, you will recall, in 1964) and respondent's memory of his 1960 vote. As one might expect, respondents disproportionately remember voting for Kennedy (61.1 percent), but the correlation between reported Kennedy vote and political trust is only .14 (compared to .39 with Johnson vote). These is *no* relationship between 1958 political trust scores and recalled vote for Eisenhower in 1956. In respect to the relationship of political trust to vote, then, 1958 appears to be a fairly typical year and 1964 a very unusual one.

If we examine the relationship between political trust and vote for the two years in more detail the situation becomes even clearer. Controlling for party identification virtually eliminates even the small relationship between distrust and Democratic vote for Congress in 1958, but the pattern in 1964 is striking. As Table 4 demonstrates, the most strongly identified partisans in

TABLE 4. T_c CORRELATIONS BETWEEN POLITICAL TRUST AND DEMOCRATIC VOTE BY PARTY IDENTIFICATION AND YEAR

Party Identification:		Strong Democrat	Democrat	Independent Democrat	Independent	Independent Republican	Republican	Strong Republican
Year	1958	−.03	.00	−.08	.03	*.37	.04	−.02
	1964	.09	.25	.20	.49	.41	.34	.12

* This deviant coefficient is a function of the Democratic votes of 6 of the 9 Independent Republicans with the lowest scores on political trust. It is difficult to assess its substantive importance.

M. Blalock, *Social Statistics* (New York: McGraw Hill, 1960), pp. 321–324.

[51] See, for example, Morris Rosenberg, *op. cit.*, pp. 690–695; Robert E. Lane, *Political Life* (Glencoe: Free Press, 1959), p. 164; and Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963), p. 285.

[52] Campbell, *et al., op. cit.*, Chs. 3, 14, 19.

[53] *Ibid.*, pp. 128–133.

[54] Presidential, as compared to Congressional, elections usually stimulate turnout. This was true in 1964 as compared to 1958, although it is not always the case. See Angus Campbell, "Surge and Decline: A Study of Electoral Change," *Public Opinion Quarterly*, 24 (1960), 397–418.

[55] The 1966 SRC election study, now available for analysis, had items comparable to those asked in 1958 and 1964 and results of a quick analysis confirm what is reported throughout the body of this article. On the particular point at issue here, Democrats (with their party in power) were slightly more trusting than Republicans ($T_c =$ .13) and the trusting voted for the Democratic candidates for Congress ($T_c =$ .16). Again the other alienation measures were totally irrelevant in predicting the voters' choices.

TABLE 5. POLITICAL TRUST BY VOTING BEHAVIOR FOR 1964 PRIMARY VOTERS BY PARTY PRIMARY

		Voters in the 1964 Primaries					
		Republican Primary			Democratic Primary		
Political Trust		Low (0–1)	Medium (2–3)	High (4–5)	Low (0–1)	Medium (2–3)	High (4–5)
General Election Voting	Goldwater	87%	74%	50%	40%	13%	7%
	Johnson	13	26	50	60	87	93
		100%	100%	100%	100%	100%	100%
		(N = 89)	(N = 51)	(N = 40)	(N = 88)	(N = 83)	(N = 127)

1964 were least affected in their voting behavior by the extent of their political distrust, while independents voted very heavily for Goldwater if they were distrustful. In this case, as with Negroes, a strong attachment to a reference group (here party) inhibited the effects of cynicism on the direction of the vote—only the least trusting Strong Democrats broke ranks to vote for Goldwater and only the most trusting Strong Republicans were able to support Johnson with their ballots.[56] It should also be noted that these relationships were stronger for the Republican identifiers than for the Democrats. This is probably because of the added pressures on these individuals to break party ranks in this election[57] as reflected here by the sensitivity of T_c coefficients to the slopes of relationships. The 1964 campaign, then, served to activate feelings of political hostility in a meaningful way for all but the most strongly identified partisans, and even these people were not totally immune.

Even primary voters were strongly affected in their 1964 general election behavior by feelings of political trust or distrust. As one might suspect, primary voters are even more strongly identified with one of the two parties than other members of the electorate[58] and party identification provides them with an even stronger guide to electoral behavior than it does other voters. As Table 5 indicates, however, voters in the Republican primaries who were high in political trust split their votes between Goldwater and Johnson in the national election, while forty percent of the Democratic primary voters who were low in trust cast their ballots for Barry Goldwater. This table illustrates very plainly the fact that a larger proportion of the Republican than Democratic primary voters were politically distrustful and shows the influence of party on vote, but it also gives a clear indication of the importance of political trust in determining voter defections in this election,[59] even among the more active members of the electorate.

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[56] It is interesting to note that *Gamma* coefficients are less sensitive to this fact, but give a better indication of the fact that a relationship exists for every group of identifiers. These coefficients are as follows:

Strong Democrat	.49
Democrat	.48
Independent Democrat	.63
Independent	.75
Independent Republican	.66
Republican	.42
Strong Republican	.45

[57] See Philip E. Converse, Aage R. Clausen and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," this REVIEW, 59 (1965), 321–337; and Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," this REVIEW, 60 (1966), 19–29, especially pp. 21–23.

[58] It should be noted that very few members of the troublesome Wisconsin electorate are represented in the 1964 SRC sample since only Sheboygan was in the sampling frame. A study done in Wisconsin (Austin Ranney and Leon D. Epstein, "The Two Electorates: Voters and Non-Voters in a Wisconsin Primary," *Journal of Politics,* 28 (1966), 598–617), however, demonstrated that even there (p. 613) "primary voters, far more than party identifiers who did not vote in primaries, merited the description 'the party faithful'" since they (primary voters) did not defect as much from their party in general elections as non-primary voters and were more active politically.

[59] Unfortunately, the SRC data do not indicate who the individual voted for in the particular primaries.

TABLE 6a: POLITICAL POWERLESSNESS AND DISTRUST BY VOTING PREFERENCE AND BEHAVIOR, 1964

Alienation:*	Powerless and Distrustful %	Powerful and Distrustful %	Powerless and Trustful %	Powerful and Trustful %
Voted Democratic	45%	43%	60%	67%
Non-voter, Democratic Preference	20	10	26	16
Voted Republican	28	43	11	15
Non-voter, Republican Preference	7	4	3	2
	100% (N=296)	100% (N=341)	100% (N=300)	100% (N=600)
No answer, Refuse or Don't Know on Voting Preference or Behavior	(N=10)	(N=3)	(N=10)	(N=8)

Without carrying this much further, it should be noted that people who engaged in activities beyond voting (attending political meetings, working in a campaign, writing letters, giving money) were more likely than others to support Goldwater in 1964, and the relationship between political trust and vote direction grew progressively stronger for the more active groups. The Republicans who could not support Goldwater tended to withdraw from active participation in the campaign. There is no evidence, however, that Goldwater was able to mobilize substantial numbers of politically distrustful voters who were previously politically inactive.[60]

Was there an interaction between political powerlessness and distrust in this election? Did individuals who saw themselves as politically powerless *and* distrustful of the national government flock to the Goldwater banner in unusual numbers? Clearly, as Table 6a shows, the powerless voted less often than the powerful in this election, regardless of their level of political trust. In addition, as both Tables 6a and 6b demonstrate, the distrustful were more likely to vote Republican, no matter how powerful or powerless they felt. In fact, individuals who felt powerless and distrustful were somewhat more likely to vote *against* Goldwater than persons who felt powerful as well as distrustful. This is the exact opposite of what those who stress an interaction between powerlessness and distrust would predict.

If a feeling of powerlessness does not act as a stimulus to voting for an extremist candidate among the distrustful, what does? While I cannot attempt to give a complete answer to this question here, the emphasis in the literature on negativism and protest among the alienated de-

TABLE 6b. POLITICAL POWERLESSNESS AND DISTRUST AMONG VOTERS ONLY

Alienation:*	Powerless and Distrustful %	Powerful and Distrustful %	Powerless and Trustful %	Powerful and Trustful %
Voted Democratic	61%	50%	85%	82%
Voted Republican	39	50	15	18
Percentages are down	100% (N=218)	100% (N=293)	100% (N=213)	100% (N=496)

* The powerful and trustful are defined as those above the sample mean (i.e., T>50).

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[60] In Stokes, *op. cit.*, there is very minor evidence of a relationship between past voting participation and political distrust, i.e., the greater the distrust, the less regular a person's voting turnout. There is no evidence of this in the Goldwater election. This not only negated a part of his electoral strategy, but goes against notions about the mobilization of the alienated non-voter in certain electoral situations.

TABLE 7: T_c CORRELATIONS BETWEEN POLITICAL TRUST AND DEMOCRATIC VOTE BY ISSUE ATTITUDE, AND YEAR

Issue	Issue Attitude (Direction)			
	Strongly Favorable		Strongly Opposed	
	1958	1964	1958	1964
1. Federal Aid to Education	.01	.21	−.17	.47
2. Government Enforced School Integration	.01	.27	−.16	.47
3. Government Enforced Equal Job Opportunity	.05	.25	−.25	.36
Foreign Aid	*	.30	*	.46
4. Trade with Communist Nations	*	.21	*	.43
Internationalism**	−.08	**	.00	**

* Question not asked in 1958.

** Questions not asked in 1964. This general item was the only close approximation: "This country would be better off if we just stayed home and did not concern ourselves with problems in other parts of the world."

serves some attention. The 1964 election heightened the tendency for people holding conservative attitudes to vote Republican,[61] but the more interesting phenomenon for our purposes is the interaction between attitudinal negativism, political distrust and protest voting which occurred in *both* 1958 and 1964 (the two years for which we have data).

Table 7 is a comparison of the T_c coefficients relating political trust and Democratic vote in 1958 and 1964 within opinion categories of representative issues. For purposes of presentation only the correlations for the strongly favorable and strongly opposed categories are given,[62] but the magnitudes of the coefficients are usually ordered across the opinion spectrum. What emerges is a very interesting pattern (with one exception) where those strongly opposing a liberal policy position tend to vote against the party in power (in this case defined as the party holding the Presidency) if they are distrustful politically. Conservatism (or at least negative attitudes) and political distrust interact to produce a behavioral negativism.

In 1958, politically distrustful people with conservative policy attitudes tended to vote Democratic and in 1964 similar individuals went heavily for the Republicans. The situation in 1964 was different, of course, in that the relationship between political distrust and vote held for those with positive (liberal) attitudes too, but the relationships here were much lower in magnitude than for the conservatives. These data suggest a modification of the hypothesis that there is a connection between alienation (at least in the form of political distrust) and protest voting. They suggest that attitudinal negativism on political issues is an important contextual factor which stimulates the relationship.[63] They show in addition, of course, that a candidacy like Goldwater's also makes political distrust behaviorally relevant for individuals with positive policy attitudes.

Certain important questions remain. First, are these issue attitudes important contextual factors in defining the relationship between other forms of alienation (or trust with a different focus) and behavior? The general answer, based on the measures available to me, is no. The only interesting differences are found in the relationship between political efficacy and vote, and here those with negative issue attitudes tended to vote disproportionately for Goldwater if they felt highly efficacious.[64] This is not only the exact opposite of the situation with political trust (since alienation is related to a Johnson vote in the efficacy case), but the magnitudes of the coefficients are exceedingly small (below .10) and it is hard to attach much substantive significance to them.

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[61] The correlations between Republican identification and issue attitudes also increased.

[62] SRC asked respondents to rate their agreement with an attitude statement on a five point Likert scale ranging from "Agree Strongly" to "Disagree Strongly" in 1958, and in 1964 asked them their opinions on an issue first and then assessed opinion intensity.

[63] The 1966 election data reveal a similar pattern.

[64] In contrast, the highly efficacious voters with positive issue attitudes tended to vote for Johnson.

A second question concerns the meaning of issue conservatism here. Are the relationships found a reflection of an interaction between deeply held attitudes or are we merely dealing with a cumulation of negative feelings, some of which are shallow in content? The fact that the very vague item on internationalism asked in 1958 was the only one which did not stimulate a relationship between political distrust and negative voting leads one to suspect that there may be something to an interpretation which puts some emphasis on the substance of the particular issues involved, but this is certainly an open question at this point.[65] What is required now is research on the structure and stability of issue attitudes for the trustful and distrustful and an investigation of the cumulative effects of negative issue attitudes as they interact with political distrust in influencing the vote.

V. POLITICAL ALIENATION AND THE POLITICAL SYSTEM: THE MODERATING ROLE OF INSTITUTIONS

In the American political system, basic issues are usually muted in elections and debate. Candidates for office seek the middle ground to gain marginal support, legislative personnel do the same to build winning coalitions, parties are composed of "wings" representing divergent points of view, etc. When this delicate system is upset, the patterns of people's responses to the stimuli presented also change. Motivations which are usually of limited importance in determining the direction of voting behavior can come strongly to the fore. This is especially true for individuals who are not very strongly committed to either of the established parties currently operating.

We have seen in the preceding pages a vivid example of the role of political institutions in moderating the effects of political hostility, and what can happen when this system does not function effectively. Goldwater's appeal was not unrecognized and did not go unheeded by large groups of people who were cynical about the personnel and operations of government. The basic issues and appeals in this election were, in one sense at least, *less* ambiguous than usual—the usual smoke screen laid down by fundamental agreement between traditional party candidates was missing—and voters responded accordingly. Party loyalties were partly set aside by basic orientations to the political system. The fact that Goldwater was so unsuccessful does not eliminate this fact, nor the possibility that a more able candidate could make better capital out of the potential manifested in this situation.[66]

It seems that the importance of a variable like political trust in governing the direction of vote is not determined solely by whether elections are structured contests between standard parties or non-structured (primaries, referendums, etc.), but by what appeals are made and how clearly they are perceived. In some cases party or group is a more relevant variable for organizing whatever perceptions exist and for making choices as to the direction of behavior than in others. The political system of the United States, as it traditionally operates, serves to dampen the relevance of attitudes like political trust, but this is not an inevitable by-product of partisan elections, even in a two-party system.

It is also aparent that attitudes of alienation are not equally relevant in each situation. Political trust was a powerful determinant of vote in 1964, but interpersonal trust, personal efficacy, and political efficacy were virtually irrelevant. The utility of speaking of alienation in terms of focus as well as form is amply demonstrated by the fact that the correlation between vote and scores on trust in people in 1964 was nonexistent ($T_c = -.01$) in spite of the extreme relevance of political trust. Goldwater's partisans, no matter what their party affiliation, were quite selective in the object of their distrust.

Evidence in the literature clearly suggests that politically inefficacious people vote negatively in referendums and there is inferential material to support the notion that the powerless are hostile to local authorities and express their distrust in this way. In 1964 there is absolutely no support for a model which holds that perceived powerlessness leads to political distrust which, in turn, leads to voting against the prevailing order (negative voting) or even for the simpler idea that attitudes of powerlessness and distrust interact in affecting behavior in national elections. The evidence is that political distrust alone (or, better, distrust interacting with negative policy attitudes) spurs negative voting in national elections, especially under certain favorable conditions which heighten the relevance of distrust to the voting choice. Feelings of powerlessness influence turnout, but *not* the voters' choices.

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[65] See Campbell, *et. al., op. cit.*, Chs. 8 and 9, and Philip E. Converse, "The Nature of Belief Systems in Mass Publics," pp. 206–262 in David E. Apter (ed.), *Ideology and Discontent* (New York: Free Press of Glencoe, 1964). Given the research reported by these scholars, an ideological interpretation must be approached with great caution.

[66] See Philip E. Converse *et al., op. cit.*, pp. 321–336. They point out how poor Goldwater's appeal was on most policy issues and how fear of his position on non-racial issues often muted the so-called "white-backlash" which did exist as a powerful potential. A more effective candidate with his views who could avoid labels like "trigger-happy" might have done considerably better. Certainly, these data indicate that an appreciable increase in the number of people holding strongly negative issue opinions would increase the vote totals of a Goldwater-type candidate.

Why wasn't perceived political powerlessness a stimulus to negative voting in the 1964 election? One possibility, I think, is the nature of the Goldwater appeal. It was against a major governmental role in the society—opposed to a backdrop of social and economic security provided or supported by government. This kind of appeal might ring true to those who distrust government, but the politically powerless would, if anything, tend to feel that they need this protection. Therefore, the finding that those who felt politically powerless and distrustful were more likely to vote for Johnson than people who felt powerful and distrustful. If a populist-type candidate were to present himself to the electorate this might be reversed. In such a setting, powerlessness and distrust could conceivably interact significantly. Survey data on Wallace voters in the 1968 election will soon be available for testing this hypothesis.

PARTY CHOICE AND SUPPORT FOR DEMAGOGUES: AN EXPERIMENTAL EXAMINATION*

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The relationship of parties to democracy and to political stability has concerned scholars for years.[1] Extant party systems have almost continually been under attack by reformers bent on achieving various objectives, among them greater party responsibility, greater honesty, and greater efficiency of governmental operation.[2] But party systems have not suffered from a lack of defenders: reformers have often been engaged in vigorous debate. Such controversy, apart from the merits of the arguments on either side, focuses attention on a number of interesting empirical propositions about parties in democratic systems. These propositions, unfortunately, have not often been put to rigorous systematic test.

For example, defenders of the American party system in particular point out that an important function of parties is the guiding of social conflict into moderate, non-revolutionary modes of expression.[3] Attempts at reform, goes the argument, should be considered as to whether they might interfere with this beneficial function. Citizens are viewed as having inherent tendencies to perform destabilizing political acts or to vote for extremist political figures. Somehow, we are told, these citizens are restrained from indulging these immoderate predispositions by the party system.[4] In a sense, the argument continues, parties save the great unwashed from themselves; all democrats should be thankful that they do and appreciate them for this service.

What empirical ground is there for believing that parties contribute to behavior patterns productive of stability and moderation in democracies? Though not abundant, there is some evidence that where parties are not highly salient for citizens, voting behavior destabilizing to the political system may take place. Where there is no voter commitment to party *qua* party, an immoderate politics seems more likely. Party may be replaced by or act as a vehicle for 1) personal candidate characteristics or 2) ideological principle. Either contingency may enhance the prospects for electoral success of high-temperature demagogues. An often-cited example of the first is to be found in the American South. In this one-party region, voters frequently are denied the opportunity to cognize meaningful electoral alternatives in terms of party. Victory in the Democratic primary has been tantamount to election. Apparently, at least in some areas, loyalty to faction has not been the functional equivalent of party loyalty. Factions engender only transient allegiances resulting in a very volatile electoral process.[5] There is a great deal of unattached political affect which can be capitalized upon by irresponsible demagogues of the moment.[6] "Perhaps a clue to he picturesque quality of southern political leaders lies in the fact that attention-getting antics function as a substitute for party . . . in the organization of support."[7]

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* The order of the authors' names should not be interpreted to imply seniority of authorship. This is a joint enterprise. The authors wish to thank the University of Kentucky Research Foundation for financial support of the research upon which this article is based.

[1] Sigmund Neumann, "Toward a Comparative Study of Political Parties," in Sigmund Neumann (ed.), *Modern Political Parties* (Chicago: University of Chicago Press, 1956), pp. 395-421; Maurice Duverger, *Political Parties* (New York: John Wiley Science Editions, 1963), pp. 116–132, 275–280; M. Ostrogorski, *Democracy and the Organization of Political Parties* (New York: Macmillan Co., 1922), Vol. II, pp. 651–662; Frank J. Sorauf, *Political Parties in the American System* (Boston: Little, Brown Co., 1966), pp. 8–12.

[2] Report of the Committee on Political Parties of the American Political Science Association, *Toward a More Responsible Two-Party System* (New York: Rinehart, 1950); E. E. Schattschneider, *Party Government* (New York: Rinehart, 1942), pp. 206–210; James MacGregor Burns, *The Deadlock of Democracy* (Englewood Cliffs, N.J.: Prentice Hall, 1963), pp. 325–332; However also see Austin Ranney, *The Doctrine of Responsible Party Government* (Urbana: University of Illinois Press, 1954).

[3] Clinton Rossiter, *Parties and Politics in America* (Ithaca, N.Y.: Cornell University Press, 1960), p. 39; V. O. Key, *Politics, Parties and Pressure Groups*, 5th. ed. (New York: Crowell, 1964), pp. 200–205; Alan Nevins, "The Strength of Our Political System," *New York Times Magazine* (July 18, 1948), pp. 5ff.

[4] Edward C. Banfield, "In Defense of the American Party System," in Robert A. Goldwin (ed.), *Political Parties, U.S.A.* (Chicago: Rand-McNally, 1964), pp. 21–39; Aaron B. Wildavsky, "On the Superiority of National Conventions," *Review of Politics*, 24 (July, 1962), 307–319.

The contention that voter commitment to party mutes ideological fervor is often found in comparisons of the American two-party system and European multi-party systems. In France, we are told, partisan conflict reflects controversy over basic questions of political community and regime.[8] Everything is a matter of principle. Voters do not develop enduring loyalties to party, but rather to positions or *"tendances."* Accordingly, parties themselves are secondary, even transient. "Flash parties," probably committed to some extreme position, threaten to disrupt the polity only to lose all electoral support and recede a short time later.[9]

Evidence is by no means firm, but there is some suggestion that non-partisan elections in the United States provide arenas for successful presentation of strident appeals. Though partisan voting loyalties often continue to intrude upon non-partisan contests,[10] such elections sometimes do provide opportunities for voters to indulge sentiments about groups for which they have negative valences without repudiating their party.[11] Structuring of the election by party is apparently replaced not by issue content, but more likely by "religious, ethnic, or other extraneous associations,"[12] perhaps with overtones of viciousness.

Finally, referenda require voters to structure balloting situations without benefit of party. Once more there is slight evidence that this kind of situation allows demagogic approaches to find support. Under some conditions, referenda produce rather high rates of participation among normally electorally inert segments of the population. Moreover, such persons demonstrate a great propensity to reject referendum proposals.[13] These phenomena are especially likely to occur in response to demagogic appeals. "When distrust of . . . the powerful generally, allegations of conspiracy or of behind the scenes maneuvering for personal gain, unrestrained personal attacks . . . characterize the opposition campaign,"[14] referenda are likely to be defeated.

Thus, though the evidence here presented is fragmentary and varied, it is reasonable to expect that where electoral choices are likely to be perceived in terms of party alternatives, the probability of successful appeals by extreme candidates will be quite low. This literature performs a very important service in bringing evidence to bear on the substantive proposition that viable parties are associated with moderate politics. However, it leaves the really interesting questions largely unanswered in that few convincing, general *explanations* of the phenomenon are offered. To be sure, the task of explanation is a formidable one. It is the modest goal of this paper to sketch out two general interpretations and offer some evidence which bears on their relative merits.

I. THE POLITICAL EXPLANATION

In this approach, parties *per se* act as causal agents in electoral choice, for they function as political educators. This explanation is manifested most clearly in the literature on party identification. The "psychological attachment" to party connoted by that term can be extremely useful for an electorate. Party becomes a beacon in a confusing political world of which each voter cannot have complete knowledge.[15]

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[5] V. O. Key, *Southern Politics* (New York: Alfred A. Knopf, 1949), pp. 46–52.

[6] In the sense that there are no relatively stable blocs of voters to whom they must defer for support.

[7] Key, *Southern Politics,* p. 46.

[8] Alfred Grosser, "France: Nothing but Opposition," in Robert A. Dahl (ed.), *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966), pp. 289–292.

[9] Philip E. Converse and Georges Dupeux, "Politicization of the Electorate in France and the United States," *Public Opinion Quarterly,* 26 (Spring, 1962), 1–24.

[10] Robert H. Salisbury and Gordon Black, "Class and Party in Partisan and Non-Partisan Elections: The Case of Des Moines," this REVIEW, 57 (September, 1963), 589.

[11] J. David Greenstone, *A Report on the Politics of Detroit* (Cambridge, Mass.: Joint Center for Urban Studies, 1961), pp. II–7, II–8.

[12] Charles R. Adrian, "Some General Characteristics of Nonpartisan Elections," this REVIEW, 46 (September, 1952), 773.

[13] John E. Horton and Wayne E. Thompson, "Powerlessness and Political Negativism: A Study of Defeated Local Referendums," *American Journal of Sociology,* 67 (March, 1962), 485–493.

[14] Clarence N. Stone, "Local Referendums: An Alternative to the Alienated Voter Model," *Public Opinion Quarterly,* 29 (Summer, 1965), 216.

[15] Angus Campbell, *et. al., The American Voter* (New York: John Wiley, 1960), pp. 120–167.

". . . [T]he parties themselves serve as the significant source of political direction for the electorate. . . . As the . . . major source of political education, they create a public image of politics as a competition between parties *per se*. . . . In such a situation, politics tends to lose its ideological character; the public is not stimulated to inform itself regarding specific policies; its political role becomes one of deciding who shall manage the government, not what shall the government do. For the bulk of the electorate, this decision is determined by long-established party loyalties. . . ."[16] The prominence of this cognitive device in electoral decisions among American voters is well established. Moreover, its role in maintaining the non-ideological, stable character of American politics is suggested by cross-national research which demonstrates the absence of widespread party identification in more turbulent political systems.[17]

Using different kinds of data and insight, other writers describe a highly positive and specific role for parties in the determination of voting behavior. Parties, once they are established, allegedly encourage, supervise, persuade, cajole, educate, electioneer, and organize electorates so that they respond to party symbols.[18] This response, we are told, entails a large number of "desirable" consequences, not the least of which is the moderation of political conflict. The rallying of popular support by parties is seen as "the functional equivalent of revolution."[19] This fortunate "sublimation" of the electorates' dangerous impulses of course means less extreme practice, less violence, and fewer demagogues.

To some extent, the political approach begs the question of why in some democracies, extant parties fail in their educative task. The French voter does not perceive his voting alternatives in terms of party to the extent that the American does. And the French are allegedly more prone to favor demagogues. But parties in France, like those in the U.S., have enjoyed a long existence. What makes for party viability, for successful education, for ability to present meaningful symbols to the electorate? Additional explanation is necessary. However, the approach does focus our attention on the possibility that parties can have an independent impact once the conditions of their viability are met. This caveat leads to consideration of the second major interpretation.

II. THE CULTURAL EXPLANATION

In this approach to the disassociation of party-defined electoral choice and success of demogogues, parties are not causal agents to all. Indeed, they are epiphenomenal. If they are associated with moderate political behavior, it is not because they condition the behavior, but because their existence and moderate behavior are both dependent upon the same cultural conditions. Once cultural orientations are such as to produce moderation, appropriate party institutions will develop to act as vehicles for political expression.

There are several scholars who stress the cultural antecedents of moderate, stable democracy, and most of them attribute to party an expressive or attendant (rather than causal) role. In an extensive five-nation study is described the "civic culture," certainly the very antithesis of a condition which would lead to the making of electoral choices on the basis of attraction to demagogues. This culture is defined by a configuration of personal feelings and attitudes. Where many people feel an obligation to participate in the electoral process, where there is a good deal of involvement in the (moderate) activities that customarily surround the selection of leaders, where the population is generally not alientated (feels efficacious), there is likely to exist little prospect for the success of extremists or "messianic movements that lead to democratic instability."[20]

But these cultural conditions also entail "patterns of partisanship." There must be some channel through which these personal feelings can "flow" to have an impact on political affairs. Partisanship provides these. The civic culture is most likely to produce "open" partisans, very moderate voters to whom the notion of party seems very central. Such citizens contrast sharply to partisans bred in non-civic cultures —on the one hand, apathetics who are withdrawn, and on the other hand, intense types or parochials for whom "partisanship is not a political phenomenon, but a cultural-religious one."[21]

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[16] Angus Campbell and Henry Valen, "Party Identification in Norway and the United States," in Angus Campbell, *et. al.*, (eds.), *Elections and the Political Order* (New York: John Wiley, 1966), p. 267.

[17] Converse and Dupeux, *loc cit.*

[18] Hugh Clokie, "The Modern Party State," *Canadian Journal of Economics and Political Science,* 15 (May, 1949), 142–143; Robert MacIver, *The Web of Government* (New York: Macmillan, 1947), pp. 211–214; E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart, and Winston, 1960), p 59.

[19] Key, *Parties, Politics and Pressure Groups,* p. 203.

[20] Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, 1965), p. 355.

[21] *Ibid.*, p. 115.

Thus it appears that the civic culture not only inhibits the success of demagogues, but creates the underpinnings for competition between viable parties, while other cultures conduce to "messianic" leaders and also lead to situations where party is either unsupported or relegated to vehicle status for other passionately felt concerns.

Lipset's famous study suggests that various "extremist" political movements are engendered by social factors such as isolation, lack of wealth, and illiteracy. To be sure, parties are not ignored. Indeed, where "parties of representation" secure most of the vote, there is likely to exist a stable democratic order. But this is primarily because both the parties and the stability are dependent upon a series of cross-cutting, non-reinforcing social cleavages (which are most likely to occur in integrated, wealthy, literate societies).[22] These "cross-pressures" produce such effects by generating a culture of "cosmopolitanism," one characterized by "tolerance," and by "reduced emotion and aggressiveness." Electoral support for party is merely the reflection of such cultural conditions.

The list of advocates of the cultural explanation could be lengthened considerably. A recent "theoretical" explication suggests that vulnerability of democratic government, which may involve citizen susceptibility to the appeals of demagogues, is due to anomie, which is in turn a consequence of "incongruity" between various authority structures in society. Thus, heavily Catholic countries are less likely to remain democracies because the authoritarian structure of religious life is incongruent with democratic political rule.[23] The "strain" thus created renders the masses more susceptible to extreme programs. Parties are not a cause of democracy, for both political parties and democracy have common antecedents. In fact, meaningful partisan elections are "practically synonomous" with democracy.[24]

III. SOME EMPIRICAL TESTS

Hypotheses. Available evidence dramatically favors neither the political nor the cultural explanation of these phenomena. If party symbols to which the electorate has been educated to respond are not present, voting behavior indeed seems to have more demagogic support potential than if such symbols are available as choice criteria. On the other hand, the amount of support for the proposition that both demagogic support potential and the presence of effective, symbol-providing parties depend upon cultural conditions is also impressive. Thus, the contrast between these two explanations may appear overdrawn. Probably not even the most fervent apologist for parties would deny that certain social conditions are conducive to stable, non-demagogue-troubled political systems, while others are inimical. Similarly, even extreme social determinists would admit that parties can become established and that measurable effects can flow independently from them.

However, this should not obscure the necessity of additional research on the role of party in demagogic choice by voters. Though extant evidence suggests some role, we certainly do not know *how great* that role is. Indeed, due to the fact that uncontrolled variables often change when party choice criteria change—for example, comparisons between partisan and non-partisan voting entails comparisons of voting for different offices or in different locations—we cannot be absolutely sure that there is a role at all.

Our assessment of the available literature leads us to posit that cultural variables do indeed have a pronounced effect on attractiveness of demagogues to voters, but that over and above this, party choice criteria have a measurable impact—an impact which declines, however, as the values of the cultural variables approach the extremes. That is, some voters may be in conditions which conduce so strongly to support for demagogues that the educative outputs of parties and their provision of symbols in elections have no effect whatever. Demagogues may be chosen whether there are party alternatives or not. By contrast, others may be so rich in "civic culture" that demagogic candidates could never be tolerated and the educative effects of party may then be redundant. Though parties might be present in the electoral situation, their independent effect on demagogic support potential would be small indeed. In intermediate cultural conditions, however, demagogues may not be unthinkable, but parties, should they be present, may provide meaningful symbols to which electorates respond in preference to demagogic appeals.

These propositions are clearly of great importance, for they deal with the conditions under which party can be an effective instrument in the determination of the kind of rule which electorates will choose, i.e., in the determination of the stability of democracy. It is to a test of these propositions that we now turn.

Method. Systematic research of the above notions requires a design in which voter choice of demagogues is always possible, in which there is variation in the presence and absence of meaningful party symbols, and in which there is cultural variation. The extreme difficulty of realizing these desiderata in a real world situation, not to mention the problems of control of variables, necessitated a quasi-experimental approach. Data were gathered from 847 residents of the Lexington, Kentucky metropolitan area. A randomized cluster sample of all housing units in the area was drawn, and this served as a basis for the selection of respondents. Several standard instruments and modes of soliciting data were employed to provide information on social, personal, and political variables.

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[22] Seymour Martin Lipset, *Political Man* (Garden City, N.Y.: Doubleday, 1960), pp. 74–77.

[23] Harry Eckstein, *A Theory of Stable Democracy* (Princeton: Center of International Studies, 1961), pp. 21–28, 35–38.

[24] *Ibid.*, p. 3.

Clearly, there is no cultural variation among the subjects in that they are not drawn from various distinct cultural units; they are indeed all from the same local community. However, this problem is obviated if we consider as the definition of culture, "that of psychological orientation toward social objects."[25] We need not assume cultural homogeneity within a given area; in fact, it may be dangerous to do so. The culture of an area is the particular distribution of patterns of these orientations. Within our sample, there is considerable variation in relevant orientations and we can therefore deal quite nicely with culture. The three psycho-cultural variables of concern to us, allegedly of great importance for the character of political support, are alienation, anomie, and sense of citizen duty.[26]

The other relevant variables, electoral selection of demagogic candidates, and opportunity to structure voting situations in terms of partisan alternatives, are somewhat more difficult to approach. The lack of general agreement as to what constitutes a demagogue makes operationalization of the concept somewhat arbitrary. Our assessment of the literature plus the necessity of experimental manipulation led us to approach the task in the following way. A pool of "extreme" programmatic statements, such as might be uttered in a campaign, was assembled by the authors. The items referred to four issue areas: the Viet Nam war, internal communist subversion, federal centralization of government prerogatives, and civil rights or racial conflict. Most of the statements were adaptions of remarks made by figures who have at one time or another, justly or unjustly, been called demagogues. The statements were then submitted to the members of the political science department of the University of Kentucky, who were asked in light of *A Dictionary of the Social Sciences'* definition, to rank the statements on a five-point scale for their degree of demagogic content. Those statements in each of the four issue areas which received the highest cumulative scores were accepted for use in the instrument. Respondents were subsequently presented with these statements which were attributed to a candidate for Congress. Their choice of this candidate over others is regarded as evidence of attraction to demagoguery. Admittedly, this scheme does not tap all of what seems to be involved in the notion of demagoguery. Here we have only the content of an appeal. Certainly demagoguery does not flourish only because of the words used. The way they are said, the intensity and frequency of the presentation, the appearance of the presenter, and possibly the nature of available sanctions, may all play a role in demagoguery. However, few observers of the phenomenon exclude content,[27] and thus it would be a denial of considerable consensus to do so. We are measuring at least one aspect of the phenomenon, and perhaps the one most amenable to systematic observation. Thus equipped with hypothetical candidates who make demagogic utterances, we established a basic experimental design as follows: (1) All respondents were forced to choose one of the four issue areas as most salient; (2) The direction of each respondent's policy preference within the selected issue area was ascertained; (3) With the appropriate policy preference reflected in the content of some of the alternatives offered, each respondent was asked in which of four hypothetical elections he would most likely participate; and (4) He was asked how he would vote in that election. Hence, each respondent was given printed information about eight candidates—opposed pairs in four hypothetical elections. The elections were characterized in the following way: *Election I*, candidates of differing personal characteristics (one "attractive" and the other "unattractive"); *Electron II*, candidates opposed in that one was "demagogic" in what he said while his opponent's only known characteristic was that he disagreed with the demagogue; *Election III*, candidates who took opposed but moderate stances on issues; and *Election IV*, candidates of different parties. Information about these candidates was rigidly controlled. Respondents knew nothing about the candidates in Election I except their personal characteristics; nothing about the candidates in Election III except their issue stances, and so on. There was no information about the candidates to which our subjects could respond except the designated features. Names were not even attached for fear of activating ethnic loyalties. In Elections II and III candidates addressed themselves to a policy area (one of four) previously ascertained to be of most concern to the respondent. Moreover, in Election II the demagogic candidate's remarks were always in the *direction* that the respondent's preferences had been determined to lie.[28] To reduce the possibility that the instrument itself would affect the choice, the information about each of the eight candidates occupied about the same number of printed lines. Interviewers read the information about each candidate aloud with the respondent to insure that all alternative choices had been cognized.

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[25] Almond and Verba, *op. cit.*, p. 13.

[26] Alienation is measured by the Survey Research Center's Political Efficacy Scale, Angus Campbell, *et al.*, *The Voter Decides* (Evanston, Ill.: Row, Peterson, 1954). pp. 187–194; anomie is measured by a version of Srole's instrument, see Edward L. McDill and Jean Clare Ridley, "Status, Anomia, Political Alienation and Political Participation," *American Journal of Sociology*, 68 (September, 1962), 205–213; The measure of sense of citizen duty is also taken from the Survey Research Center's arsenal, Campbell, *et. al.*, *The Voter Decides*, pp. 194–199.

[27] Allen Bullock, *Hitler: A Study in Tyranny* (New York: Bantam Books, 1961), especially pp. 43–46; Ernst Kris and Nathan Leites, "Democratic and Totalitarian Propaganda," in Leonard Fein (ed.), *American Democracy: Essays on Image and Realities* (New York: Holt, Rinehart and Winston, 1964), pp. 55–60; and Lipset, *op. cit.*, Chapter 5.

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[28] For example, a respondent who indicated that racial conflict was the most salient of the four issues, and who further indicated that his personal position was pro-Negro, was presented with a hypothetical demagogic candidate in election II to whom the following statement was attributed: "Every white man in the south is an uncivilized dog. His record of lynchings proves it. There is no white man's justice; justice comes only when the black man takes it by force." A person choosing the same issue but indicating an anti-Negro stand was presented with a hypothetical demagogue who said, "We have a right to keep niggers from moving in next door to us and we have a right to use force to do it. We must preserve western Christian civilization. Would you want your daughter to marry a nigger?" The statement attributed to a demagogue was not always the direct counterpart of that attributed to the demagogue on the opposite side of the same issue, nor was it necessarily directly parallel to the "less extreme" statement attributed to an issue-oriented candidate in election III. This was due to the fact that our operation for generating demagogic statements sometimes produced items with less specificity of reference than those used for issue-oriented candidates. On the ground that our experts seemed to believe it was the nature of demagoguery to avoid issue-specificity, we did not interpose our own judgement. As an example of the format of the electoral choice instrument, we present the entire configuration of options offered to respondents who choose Vietnam as the most salient issue, and who indicated a pro-involvement posture. The complete instrument can be obtained from the authors.

ELECTION #1

Candidate "A" is in his early thirties and has a reputation for energy and drive. He is married, has three children, and is active in his church. He is a lawyer. He recently received an award for distinguished service to his community.

Candidate "B" is a middle aged bachelor who is employed with a business firm which recently moved to Lexington from Pennsylvania. It is rumored that he is secretly engaged to a woman twenty years younger than himself.

ELECTION #2

Candidate "C" says, "The only language the communists understand is war. Your children will be safe only when our armies have killed every last communist. The Lord is on our side in Viet Nam. To fight on to total victory is the wish of the blessed Saviour."

Candidate "D" says, "One thing about this campaign is clear. My opponent and I are in very great disagreement on almost every point. The voters must choose between two programs. Mine is the most beneficial for all citizens."

ELECTION #3

Candidate "E" says, "Because of the necessity of containing communism, we must remain in Viet Nam. It is simply smart world politics to stop the efforts of the expansionist powers. If they succeed, they will go after more."

Candidate "F" says, "We must weigh the cost of staying in Viet Nam against what we gain. I believe that the costs are too high and the gain too small. I propose that we cut our costs by withdrawing from this conflict."

ELECTION #4

Candidate "G" is a *Democrat*. He has stated that he respects the principles of his party and believes that Democrats can be important influences in achieving excellent government for all citizens.

Candidate "H" is a *Republican*. He has stated that he is impressed with the prospects for his party in the years ahead, and has pledged to make a good image for Republicans in all that he does.

Manipulation of the opportunity to structure electoral alternatives in terms of party was accomplished by denying a random half of the sample access to the hypothetical partisan election. This half, hereafter called the experimental group was required to choose among three elections and six candidates, selecting on the bases of candidate personal characteristics, issue position, or demogogic features. The remaining half, the control group, of course received partisan stimuli, which, given its probable life-long exposure to the "educative" efforts of Democrats and Republicans, should have been meaningful.[29]

Although this design controls many potentially contaminating variables and allows considerable manipulation, it has certain liabilities. Obviously, the respondents are not actually politically supporting demagogus by their hypothetical voting choices nor are they actually operating in party or non-party contexts. They are participating in an experimental situation. This, however, is not an overpowering defect. A great deal has been leaned about human behavior through psychological experimentation for example. The truly remarkable thing is how little experimentation is used in political science.[30] The advantage of being able to abstract concepts for research—especially such basic ones as are discussed here—outweigh the consideration that we are not directly observing overt participation in the political process.[31]

The viability of our procedures has been established elsewhere.[32] Suffice it to say here that response patterns carry no hints that subjects treated their hypothetical choices capriciously. Many of the emergent data are thoroughly consistent with what has been found in studies of actual electoral behavior.

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[29] This technique is similar to the "split ballot" discussed in Hadley Cantrill, *Gauging Public Opinion* (Princeton: Princeton University Press, 1944), pp. 23ff. Split ballots, by presenting different wordings or alternate forms of items to separate but matched subsamples, reveal response differences which are artifacts of instrumentation. In this study, such response differences, due to alternate instrumentation, are used as a measure of the dependent variable. Sound experimental research, of course, requires that there be no relevant contaminating differences between experimental and control groups. We sought to achieve this by random assignment of respondents. That we were successful is demonstrated in the table below. Tests for differences were performed on several variables. In no case was there anything approaching a significant difference.

COMPARISON OF EXPERIMENTAL AND CONTROL GROUPS

Variable	Test
alienation	$D=.058$
anomie	$D=.035$
sense of citizen duty	$D=.080$
party identification	$D=.084$
education	$D=.074$
income	$D=.054$
sex	$Z=.035$
race	$Z=.008$

Where data are in ordinally arranged categories, the Smirnov two-sample test is employed. Dichotomous data require the difference of proportions test. In all cases, $p > .50$. Obviously, these variables could produce no observed differences in behavior.

[30] Many scholars including philosophers of science urge experimentation on the study of politics despite the obvious difficulties of implementation in field situations. The advantages of manipulation and control of variables and of relatively easy replication outweigh these considerations. H. J. Eysenck, *The Psychology of Politics* (New York: Frederick A. Praeger, 1955), p. 252; Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler Publishing Co., 1964), pp. 145–147; Fred N. Kerlinger, *Foundations of Behavioral Research* (New York: Holt, Rinehart and Winston, 1964), pp. 359, 375–391; John R. P. French, Jr., "Experiments in Field Settings," in Leon Festinger and Daniel Katz (eds.), *Research Methods in the Behavioral Sciences* (New York: Holt, Rinehart, and Winston, 1953), pp. 98–135; Samuel A. Stouffer, "Some Observations on Study Design," *American Journal of Sociology*, 55 (January, 1950), 355–361.

[31] Some scholars have successfully employed experimental method in the study of politics. One work employing a design especially similar to ours is Leon J. Kamin, "Ethnic and Party Affiliations of Candidates as Determinants of Voting," *Canadian Journal of Psychology*, 12 (March, 1958), 205–213. Other experimental studies of political behavior include the works of Theodore Becker, most of which builds upon his *Political Behavioralism and Modern Jurisprudence* (Chicago: Rand-McNally, 1964); see also J. A. LaPonce, "An Experimental Method to Measure the Tendency to Equibalance in a Political System," This REVIEW, 60 (December, 1966), 982–993.

[32] See the authors' forthcoming "Alienation and Support for Demagogues," *Polity*, 1 (June, 1969).

Findings: The Political Explanation. Do party symbols in fact independently contribute to the unattractiveness of demagogic candidates? This question can be approached by manipulating party symbols in the electoral situation and by comparing those persons for whom party is very salient with those for whom it is not.

We accomplish the first by comparing our experimental and control groups with respect to propensity to choose demagogic candidates. The denial of partisan electoral alternatives to our experimental group appears dramatically to increase their attraction to demagogues (Table I). Indeed, these results are very consistent with those in the literature on non-partisan elections and referenda. Our confidence in the implications of this literature is increased, however, because of the tighter control of possibly contaminating variables in this quasi-experimental context.

TABLE I. EFFECT OF PARTISAN ALTERNATIVES ON CHOICE OF DEMAGOGUES

	Experimental Group (N = 400)	Control Group (N = 427)
Demagogic choice coefficient*	.53	.33

* Demagogic Choice Coefficient (DCC) is simply *fo/fe* calculated from the distribution of electoral choices within the two groups. That is, since there were six discrete options available to members of the experimental group, random distribution would place one-sixth of the 386 subjects responding or 64.3 in the demagogic category. Only 34 subjects were observed so to choose, however. Hence the DCC for the experimental group is 34/64.3 or .53. The control group had eight electoral options owing to the addition of two partisan choices. Thus, *fe* of demagogic choice is one-eighth of the 418 subjects responding or 52.2. However, *fo* = 17; hence DCC = 17/52.2 = .33. The use of such calculations rather than raw percentages is necessary because of the disparity in the number of choices available. The raw percentages of respondents choosing demagogic candidates, though their absolute values are of relatively little meaning given their heavy dependence both upon the number and kind of other electoral choices available, are for the experimental group, 9%; for the control group, 4%.

Second, do those persons who do not identify with a political party, who generally reject partisan criteria for electoral choice, show a greater propensity to choose extremist candidates? Like the respondents of the experimental group, we would expect independents to operate in a condition—albeit self-imposed—which is devoid of party choice criteria. If so, they should indeed be more attracted to demagogues than those who identify with party. However, Table II reveals that this is not the case. In fact, the independents are less attracted than either Democrats or Republicans. Insofar as these data are concerned, this political explanation fails. The cultural attributes of the Republicans (e.g., as determined by sense of citizen duty or alienation), partially dependent upon social variables such as wealth and education, may help to explain the fact that their DDC is lower than that of the Democrats. However, a similar cultural explanation cannot be mustered to account for the low DCC of the independents. Although the independents of our sample are neither as politically inert nor as politically alientated as the sterotypic independent one envisages from contemporary voting studies, neither are they rich in civic culture.[33] Indeed, we have no data to explain the anomaly of the independents.[34]

TABLE II. EFFECT OF PARTY IDENTIFICATION ON CHOICE OF DEMAGOGUES

	Democrats (N = 428)	Independents (N = 198)	Republicans (N = 197)
Demagogic Choice Coefficient	.49	.36	.41

The political explanation fares a little better when we consider the effect of denial of party alternatives together with party identification. Such denial of course increases the propensity to choose demagogues of both party identifiers and independents (Table III). However, the effect is least visible among independents and most pro-

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[33] Though there are problems of rapidly diminishing cell size, attempts at controlling the DCC-party identification relationship for cultural attributes generally reduces slightly the magnitude of Republican-Democratic differences; the independents, however, retain the lowest DCC scores. That the DCC's of the party identification groups are not mere reflections of distributions of cultural or social attributes is evident from the following data. The party identification groups are remarkably homogenous. Republicans are of slightly higher social status and approximate the civic culture more closely than Democrats or independents. However, these differences attain statistical significance on only two of six variables.

[34] We suspect that there is a political explanation

TABLE III. PARTISAN ALTERNATIVES, PARTY IDENTIFICATION, AND CHOICE OF DEMAGOGUES

	Experimental Group			Control Group		
	Democrat (N=216)	Independent (N=90)	Republican (N=78)	Democrat (N=202)	Independent (N=106)	Republican (N=114)
Demagogic Choice Coefficient	.61	.40	.47	.32	.30	.36

nounced upon Democrats. This is in line with what we would expect; since party is less salient to independents, the non-availability of partisan alternatives should have relatively little impact on the criteria they employ in electoral choice. The dramatic increase in the propensity of experimental group Democrats to choose demagogues and the relatively small parallel increase among Republicans is undoubtedly due to the traditional Democratic dominance of the area. Republicans have been conditioned to rely less on party label for political guidance. They may rely on other electrical criteria, and hence denial of partisan alternatives is not a highly disorienting experience. Democrats, on the other hand, have been steeped in party symbols and hence should rely upon them more. Sudden removal of this structuring variable would understandably disorient them. This disorientation appears to result in increased choice of demagogic electoral alternatives.

It is clear that the existence of party symbols decreases the likelihood of voters choosing demogogic candidates. The political explanation of the simultaneous presence of democratic stability and viable parties is supported by these data. Despite the fact that our expectations about party identification were not met, the existence of partisan alternatives undeniably affected the behavior of our subjects. Party seems in and of itself a causal agent in decreasing attraction to demagogues and thus promoting stability.

The Cultural Explanation. As noted above, much literature suggests that political stability, and hence the unattractiveness of demagogues, is dependent upon cultural antecedents. Though —somewhat at variance with this literature— our data suggest an independent effect of parties, they do not preclude the possibility of cultural variables having a simultaneous impact. Various works suggest that a cluster of psychological orientations toward political objects is at the root of stable political order. This cluster is composed of a large number of variables— among them, certainly, alienation, anomie, and

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for the independents' low DCC, but of a slightly different kind. Evidence collected by Malcolm Jewell suggests that some southern independents may be in a transitional stage between Democratic party identification and Republican party identification. Such persons are dealing with their dissatisfaction with the Democrats decidedly within the partisan context. Consequently, demagogues may not be attractive to them.

Mean Values on Cultural and Social Variables of Party Identification Groups

	Democrats (N=428)	Independents (N=198)	Republicans (N=197)	Kruskal-Wallis Test, Value of H
Alienation	3.04	3.04	2.88	2.68, $p > .05$
Anomie	2.55	2.46	2.22	8.90, $p < .05$
Sense of Citizen Duty	4.01	3.97	4.16	5.76, $p > .05$
Campaign Activity Index	1.84	1.77	1.95	3.77, $p > .05$
Education*	5.02	5.38	5.47	10.66, $p < .01$
Income*	2.98	3.09	3.37	5.44, $p > .05$

* The means for these variables are calculated from the ordinal numbers assigned to categories into which respondents classified themselves.

feelings of obligation to participate in politics. Thus, we would expect persons who exhibit low levels of alienation and anomie and an acute sense of citizen duty to eschew demagogic electoral alternatives.[35] We investigated this proposition by examining the relationship of these three cultural variables to choice of hypothetical demogogic candidates. Table IV reveals that our expectations are met. In fact, with the distribution of scores on the independent variables trichotomized, there is a monotonic progression of the Demagogic Choice Coefficient in the anticipated direction for all three variables.

TABLE IV. EFFECT OF CULTURAL VARIABLES ON DEMAGOGIC CHOICE COEFFICIENTS

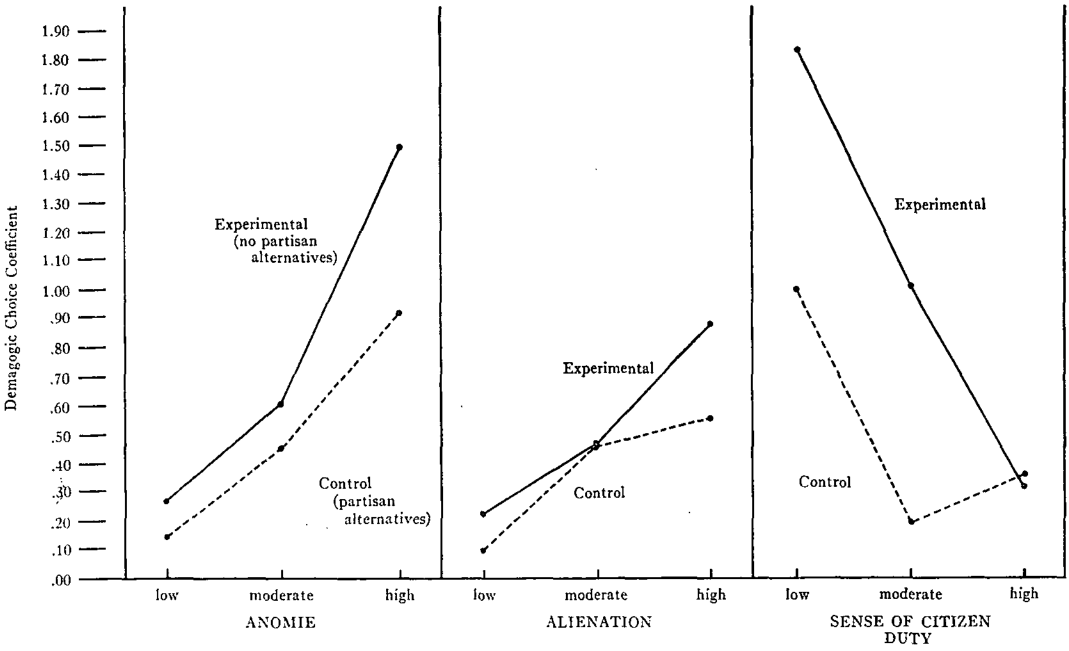
	Low	Moderate	High
Anomie	.21	.52	1.21
Alienation	.16	.43	.70
Sense of Citizen Duty	1.53	.61	.28

Hence, attraction to demagoguery, in addition to being dependent upon party, is partially explained by cultural variables. Since we know by virtue of our experimental controls that the effects attributed to party cannot be due to the uncontrolled presence of these cultural variables, it appears that both the political and cultural

TABLE V. EFFECT OF CULTURAL VARIABLES AND PARTISAN ALTERNATIVES ON DEMAGOGIC CHOICE COEFFICIENTS

	Experimental Group			Control Group		
	Low	Moderate	High	Low	Moderate	High
Anomie	.26	.59	1.47	.14	.42	.89
Alienation	.22	.43	.83	.10	.43	.51
Sense of Citizen Duty	1.89	1.00	.27	.97	.15	.29

FIG. 1. The Interaction of Cultural Variables and Partisan Alternatives in Their Effects Upon Demagogic Choice.



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[35] In additions to suggestions in Almond and Verba, *op. cit.*, clear evidence confirming this expectation is found in the authors' "Alienation and Support for Demagogues," *op. cit.*

explanations have elements of validity. Neither explanation can be discarded in its entirety. It thus remains to be asked just how these two sets of variables interact to produce their effects upon attraction to demagogues. It is clear that the party choice variable operates among persons of all cultural orientations (Table 5).

We posited that the availability of partisan electoral alternatives would have its smallest effect when values of the cultural variables approached extremes. Anomic, alienated persons who are weak in feelings of civic obligation may be beneath a cultural threshold where partisan symbols can be meaningful. On the other hand, persons with opposite cultural complexions may find the educative effects of party superfluous. The existence of partisan symbols should operate most effectively on those persons who are between these cultural extremes. An examination of Table 5 reveals that this is not the case. The graphing of these data (Figure 1) clearly shows an unanticipated interactive pattern. Instead of finding that removal of party produced the greatest effects at moderate cultural levels, we note that the largest impact is at one extreme. While persons rich in civic culture appear to be so disinclined to choose demagogues that party is largely redundant, it is definitely not the case that citizens of opposite orientations are beneath a level where partisan symbols are meaningful. Instead the relationship is linear: the more cultural variables conduce toward choice of demagogues, the greater is the sensitivity of this choice to the presence of party symbols.

IV. CONCLUSION

There is some evidence that where voters are able to respond to party symbols, very moderate political behavior takes place. This phenomenon has been attributed to 1) the educative effects of parties *per se* (the political explanation) and 2) to the holding of particular "psychological orientations toward social objects" by citizens (the cultural explanation). Advocates of the first seem to feel that parties inhibit voters from indulging immoderate predispositions, while defenders of the second would argue that moderation (and viable parties) are dependent upon the *absence* of any such predispositions. This paper, by demonstrating that voters' propensities to choose demagogic political candidates is dependent both upon availability of party symbols and upon cultural variables, lends support to both arguments. In addition, it attempts to specify the cultural conditions under which party symbols can be particularly effective variables.

The number of efforts directly and systematically to investigate the roots of political stability and instability is small. Despite the inaccessibility of behavioral manifestations of these conditions (like the attractiveness of demagogues), attempts should be made to research them. Though the findings of this paper do not confirm all of our expectations and though the relationships shown are not all startlingly dramatic, they do demonstrate the feasibility of quantitative inquiry into these kinds of classic political concerns.

DETERMINANTS OF INNOVATION IN ORGANIZATIONS*

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The present study is an attempt to identify the determinants of innovation in public agencies, i.e., the degree to which they adopt and emphasize programs that depart from traditional concerns. Innovation is suggested to be the function of an interaction among the motivation to innovate, the strength of obstacles against innovation, and the availability of resources for overcoming such obstacles.

The significance of the research can be viewed in terms of Hyneman's observation nearly twenty years ago that bureaucratic agencies ". . . may fail to take the initiative and supply the leadership that is required of them in view of their relation to particular sectors of public affairs. . . ."[1] His concern was the responsiveness of the public sector not only to expressed wants but to public wants that may go unexpressed, or be only weakly expressed, and whose utility is much more easily recognized by the informed bureaucratic official than by the ordinary citizen.[2]

While the results and conclusions to be reported appear to be largely valid for organizations in general, the empirical focus will be local departments of public health which, as a class, have had a rather dramatic succession of opportunities to respond to new public problems over the past twenty-five years. A brief introductory paragraph will orient the reader to the applied setting.

By the early 1940's—the end of the "traditional" period in public health programming from the viewpoint of this study—the American Public Health Association had defined the task of the local health department essentially in terms of a basic set of six services: vital statistics, basic sanitation, communicable disease control, laboratory services, maternal and child health services, and health education.[3] Since the middle 1940's, infectious diseases (which these services were designed to combat) have declined radically as a health problem. In the last two decades, the professional leadership has increasingly called upon local health departments to turn their attention to the control of chronic diseases, the prevention of accidents, the provision of mental health services, the control of the quality of water and air, to dental health needs and to a host of additional non-traditional concerns. Many local public health organizations have responded to these newer concerns as advancing technology has provided ways of meeting them. However, it appears that while some departments have indeed reacted innovatively, a great many others continue to pursue the traditional objectives, if not exclusively, then certainly with primary emphasis.

The study group for the present research included all full-time local health departments in Illinois, Michigan, New York, Ohio, and Ontario, serving a jurisdiction no greater than 600,000 in population, whose chief executive—the local health officer—had occupied his current position during the entire period of 1960–1964. This group comprised 94 agencies. Since one health officer refused to participate in the study, ninety-three elements were provided for analysis. The data were collected primarily by interviews with the local health officers during the summer of 1965.

I. THE CONCEPT OF INNOVATION

Because the term innovation has been employed so widely and ambiguously, it is essential

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[1] Charles S. Hyneman, *Bureaucracy in a Democracy* (New York: Harper and Brothers, 1950), p. 26.

[2] For a much more recent treatment of bureaucratic innovation and public responsiveness, see Herbert A. Simon, "The Changing Theory and Changing Practice of Public Administration," in Ithiel de Sola Pool (ed.), *Contemporary Political Science: Toward Empirical Theory* (New York: McGraw-Hill Book Co., 1967), pp. 86–120. "The 'power' to innovate," says Simon (p. 106), ". . . is probably the principal power of the bureaucracy in the realm of policy and value."

[3] Haven Emerson, *Local Health Units for the Nation: A Report* (New York: The Commonwealth Fund, 1945), p. 2.

to specify at the outset how the concept will be used and how it is related to other usages. Innovation will be defined here as *the successful introduction into an applied situation of means or ends that are new to that situation.* Alternative and generally more inclusive definitions have been offered in the past.[4] For example, there has been a frequent tendency to combine the idea of adoption or adaptiveness with the idea of invention; occasionally, the term "innovative" has been assigned to mean exclusively what is more generally called "inventive" or "creative."

It seems important at the present stage of research and theory to separate the idea of invention from the idea of innovation. Invention implies bringing something new into being; innovation implies bringing something new into use.[5] In the organizational world this distinction is particularly important, for we are interested at times in whether an organization can create something new for its own use or for exploitation by others, and at other times in whether an organization can successfully adopt goals or processes or policies that are new in the sense of being departures from its own tradition.

There appears to be a good deal of agreement now on some of the factors that most enhance inventiveness or creativity in organizations, including the availability of individuals capable of producing new ideas[6] and the development of an organizational pattern that maximizes flexibility and opens lines of communication.[7] Unfortunately, less unanimity can be reported on the correlates of organizational innovation. There has been much more empirical research in this area, but it consists of scattered projects representing different disciplines, motivated by different considerations, and employing a strikingly heterogeneous selection of independent variables.

If any one group of variables may be said to stand out among all others as empirically determined correlates of innovation, it is the group of interrelated factors indicating size, wealth, or the availability of resources. Mansfield,[8] Mytinger,[9] Hage and Aiken,[10] Eisenstadt,[11] and Rogers[12] all conclude that organizational size and wealth are among the strongest predictors of innovation in the sense of readiness to adopt new patterns of behavior. Other organizational characteristics have also been identified as predictors of innovation in this sense, including informality, complexity, and decentralization in organizational structure,[13] breadth of organizational goals, and absence of dominance by a single professional ideology.[14]

In addition to the organization itself, the environment of the organization appears to be extremely important in two ways. An organization may be more likely to innovate when its environment is rapidly changing than when it is steady. In this sense, "environment" includes such factors as market conditions, technological changes, clientele needs and demands, and the labor market. Burns and Stalker count this variable heavily as determining whether or not the firms they studied adopted new, more organic management techniques.[15] In addition, innovation should also be more likely when the *social* environment to which an organization (or an individual) belongs has norms that favor change than when its norms do not favor change. In this regard, Rogers reports research in which fairly strong correlations were found between innovation and the norms of the relevant community, placed on a dimension from "traditional" to "modern."[16]

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[4] See Victor A. Thompson, "Bureaucracy and Innovation," *Administrative Science Quarterly*, 10 (June, 1965), p. 2; Everett M. Rogers, *Diffusion of Innovation* (New York: Free Press of Glencoe, 1962), p. 308; James Q. Wilson, "Innovation in Organization: Notes Toward A Theory," in James D. Thompson (ed.), *Approaches to Organizational Design* (Pittsburgh: Univ. of Pittsburgh Press, 1966), p. 196; Homer G. Barnett, *Innovation* (New York: McGraw-Hill Book Co., 1953), p. 7.

[5] The same general distinction has been made by others. *Cf.*, Rogers, *op. cit.*, pp. 195–196, and Simon, *op. cit.*, p 107.

[6] See Gary A. Steiner (ed.), *The Creative Organization* (Chicago: The University of Chicago Press, 1965), pp. 16–18.

[7] Harold Guetzkow, "The Creative Person in Organizations," in Steiner, *op. cit.*, pp. 35–45; Tom Burns and G. M. Stalker, *The Management of Innovation* (London: Tavistock Publications, 1961), pp. 85–86, 89, 121–122; Thompson, *op. cit.*, p. 12.

[8] Edwin Mansfield, "The Speed of Response of Firms to New Techniques," *Quarterly Journal of Economics,* (May, 1963), 293–304.

[9] Robert E. Mytinger, *Innovations in Public Health* (unpublished doctoral dissertation, Univ. of California at Los Angles, 1965), p. 212.

[10] Jerald Hage and Michael Aiken, "Program Change and Organizational Properties: A Comparative Analysis," *American Journal of Sociology,* 72 (March, 1967), pp. 516–517.

[11] Samuel N. Eisenstadt, *The Political Systems of Empires* (New York: The Free Press of Glencoe, 1963), pp. 27, 33–112.

[12] Rogers, *op. cit.*, pp. 40, 285–292.

[13] Hage and Aiken, *op. cit.*, pp. 503–519.

[14] Mayer N. Zald and Patricia Denton, "From Evangelism to General Service: The Transformation of the YMCA,"*Administrative Science Quarterly*, VIII, No. 2 (September, 1963), p. 234.

[15] Burns and Stalker, *op. cit.*, p. 96.

Each of the foregoing correlates of innovation must be viewed as an attribute of a collectivity —an organization. To complete the list, numerous variables measured at the individual level have also been found to relate to the type of innovative change of interest to us here. It would appear from a review of the literature that chief among these are the attitudes of an individual toward change, or any ideology he may have that would influence a specific type of innovation. Such factors were found to be important by Blau, Fliegel, Rogers and Eisenstadt.[17] In addition, the "cosmopoliteness"[18] of an individual is reported to be a significant correlate of innovation by Mytinger and by Rogers.[19] Blau found both the competence of an individual and his material and status interests to be associated with innovation.[20] A positive professional orientation was found by Rogers to be associated with innovation, as was opinion leadership status within a relevant communications network.[21]

It seems fairly clear, then, that the determinants of invention and the determinants of innovation in organizations are not identical. Inventiveness seems to be affected most by individual creativity and by the degree of hierarchical informality in organizational structure. Innovation, on the other hand, has been linked to size, wealth, environment, ideology, motivation, competence, professionalism, non-professionalism, decentralization, opinion leadership, and still other variables. Because of this difference, it appears highly desirable to distinguish innovation from invention in research.

If the preceding discussion is considered in light of the definition of innovation offered above, it is clear that we have been elaborating upon the word "introduction" and upon the sense of the phrase "new to that situation." We have, in other words, emphasized that innovation is meant to exclude creativity *per se* and to include the notion of adopting something non-traditional whether it was invented within or outside of the organization concerned.

The term "successful," like "introduction," has important implications for the measurement of innovation. The successful introduction of some new method or goal implies its acceptance by the individual or group that constitutes the human element in a pertinent applied situation. If a health department tentatively introduces a new program, action of the department to increase emphasis on the program over time continues to be innovative behavior until the change has been completely accepted by organizational personnel, the public or other relevant groups. Such action is no longer innovative, of course, when the idea has become part of the organization's tradition.

The same point has been made in connection with innovation by individuals, but it is especially significant in organizational behavior, where we have long recognized and been concerned with the phenomenon of resistance to change.[22] It is important to note that both organizations and individuals may adopt a practice and then discontinue it for one reason or another or maintain it only on a token level. Rogers reports some studies in which discontinuance rates were found to be quite high.[23] It may help us to gain insight into innovation, therefore, if we do not restrict ourselves to its usual operational definition, the simple adoption of new practices, but include also a definition that allows for increased emphasis upon non-traditional programs recently introduced.[24] For this reason and others to be noted subsequently, two basic working measures of innovation will be employed in the analysis. One is the total number of non-traditional services adopted by the department. The other is the total number of personnel units (measured in man-years or the equivalent in dollars) that were added in all non-traditional program areas during the five-year period 1960–1964. This latter view of innovation will be labeled "progressive programming."

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[16] Rogers, *op. cit.*, pp. 285–292.

[17] Peter M. Blau, *The Dynamics of Bureaucracy* (2d ed. rev.; Chicago: Univ. of Chicago Press, 1963), p. 246; Rogers, *loc. cit.;* Frederick C. Fliegel, "A Multiple Correlation Analysis of Factors Associated with Adoption of Farm Practices," *Rural Sociology,* 21 (September-December, 1956), pp. 288–289, 291; Everett M. Rogers, "A Conceptual Variable Analysis of Technological Change," *Rural Sociology,* 23 (June, 1958), pp. 139–140, 143–145; Eisenstadt, *loc. cit.*

[18] Robert K. Merton, *Social Theory and Social Structure* (Glencoe: The Free Press, 1949 and 1957), pp. 387–420; Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles," *Administrative Science Quarterly,* II (December, 1957), pp. 281–306, and (March, 1958), pp. 444–480.

[19] Mytinger, *op. cit.*, p. 195; Rogers, *Diffusion of Innovation, op. cit.*, pp. 285–292.

[20] Blau, *op. cit.*, p. 246.

[21] Rogers, *loc. cit.*

[22] See Harold J. Leavitt, "Applied Organizational Change in Industry: Structural, Technical, and Human Approaches," *Handbook of Organizations,* ed. James B. March (Chicago: Rand McNally & Company, 1965), pp. 1144–1170, for a valuable summary and critique.

[23] Rogers, *Diffusion of Innovation, op, cit.*, pp. 88–93.

[24] See the introductory paragraphs, above, for examples of traditional and non-traditional programs. The primary sources for determining the precise composition of the two lists were "An Official Declaration of Attitude of the American Public Health Association on Desirable Standard Minimum Functions and Suitable Organization of

The term "new" appears almost invariably and as a matter of course in definitions of innovation, but it has an implication that should not be taken for granted. Innovation is difficult because it involves doing something new. The introduction of innovative practices into a social setting implies actions that entail a certain amount of uncertainty, risk, or hazard. This, then, suggests one significant factor that may help to explain innovation, i.e., there are certain obstacles or deterrents to innovation that may be more or less operative in any given case.[25] These are, first, the cost of such things as materials, time, or skills, and second, the power of human forces and fears, individual and social, that may be arrayed against the introduction of new means or ends. The latter category may include the power of tradition and social values, the power of individuals and groups who may be threatened by an innovation, and the power of one's concern for his own safety, security, and self-esteem, as well as the security of people who are important to him. The more staunchly these forces are arrayed against a particular innovation, the more difficult will it be for that innovation to take place, other things being equal. Furthermore, when one thinks of "newness" in these terms, it is not the absolute or objective sense of the word that counts, but rather the relative newness of an idea to a given role or set of roles, for it is in that context that obstacles to innovation will frequently arise. Thus, the innovations we will consider here are not necessarily new to the professional leadership in the field of public health, but they are indeed innovations for the local community, the organization, or the small clique of fellow health officers or local physicians.

Although obstacles may generally be expected to inhibit innovation, previous studies indicate that other factors may stimulate or enhance it. One individual or organization will usually have greater motivation than another to adopt a new idea. In addition, one may have greater resources than another, including not only the money and skills to overcome obstacles of expense, but also resources such as a position of authority, a charismatic effect, the support of prestigious individuals and self-confidence to overcome obstacles presented in terms of human forces.

On the basis of the foregoing considerations, the following three-dimensional hypothesis is specified: *Innovation is directly related to the motivation to innovate, inversely related to the strength of obstacles to innovation, and directly related to the availability of resources for overcoming such obstacles.*

It is thus possible to specify in more general terms why some of the many independent variables covered by previous studies were related to innovation; each indicated either a relative absence of obstacles or a relative presence of motivation or resources. Environmental changes and demands, for example, frequently constitute an important source of motivation, as do material and status interests and certain relevant ideologies. "Traditional" community norms, personal attitudes generally unfavorable to change, worker resistance to change, narrow organizational goals, lack of information, mechanistic decision structures—all of these are examples of obstacles to innovation. Competence and wealth are significant resources for innovation.

Having defined innovation and presented a theoretical framework in which to consider it, let us turn now to the findings of the research and an examination of the specific determinants considered. We shall have a two-fold purpose: a) to explain a substantial proportion of the innovation studied, and b) to arrive at a better understanding of the nature of the explanatory model itself.

II. MOTIVATION

Before constructing the interview schedules, four exploratory interviews were conducted with highly placed public health professionals. These interviews disclosed a consensus on the importance of attitudes of the health officer for the innovativeness of local health departments. Specifically, the health officer's public health ideology and his inclination toward activism were considered to be the best indicators of the likelihood that his organization would introduce and

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Health Activities," *American Journal of Public Health,* 30 (September, 1940), 1099–1106; and Harry S. Mustard, *Government in Public Health* (New York: The Commonwealth Fund, 1945), pp. 128, 140–182.

[25] Cf., Mytinger's discussion of "barriers" to innovation. Robert E. Mytinger, "Barriers to Adoption of New Programs as Perceived by Local Health Officers," *Public Health Reports,* 82 (February, 1967), 108–114.

emphasize non-traditional programs. Ideologically, a "conservatism-liberalism" dimension concerning the proper breadth of local public health programming was considered important. In addition, a health officer was considered more likely to innovate if he was a "go-getter," "aggressive," "ambitious," "interested in accomplishment," and "willing to stick his neck out." These opinions expressed in the exploratory interviews were supported by prior research, which both relates innovation to similar attitudes conducive to change and emphasizes the substantial importance of leadership motivation and behavior. Thus, the "activism" and the "ideology" of the health officer, taken together, were selected to indicate motivation to innovate.

Activism is more precisely defined as the health officer's perception of the extent to which the role of local health officer requires interaction with others, especially outside the health department, to obtain ideas, support, approval, and resources for departmental programs. The attitude was measured by a 23-item Likert-type instrument that was completed by the respondent in the presence of an interviewer. The individual items were designed to measure opinions regarding the desirability of four different kinds of role activity: a) attempts to influence the health power structure in the community, i.e., higher administrative and legislative officials, the local medical society, and influential community leaders, b) attempts to obtain support, such as grant support, beyond local appropriations, c) relative emphasis to be placed upon inter-agency and public related affairs as opposed to intra-agency activities, and d) attempts to seek out community problems as opposed to waiting for them to be pointed out by others. It should be repeated that "activism" as measured is an attitude or perception rather than a demonstrated behavior.

Public health ideology is more precisely defined as the health officer's opinion regarding the scope of services that should properly be offered by the local public health agency (as distinguished from local private and voluntary enterprise) in non-traditional public health program areas. The great majority of full-time local health officers in the area covered by this research are physicians, most of whom have had years of experience in private practice. These individuals are highly likely to feel conflicting ideological pulls from their dual identification with the medical profession and the profession of public health, the one allocating to the private sphere all medical care except those programs specifically or traditionally relinquished to government by organized medicine, and the other allocating to the public sector all unmet health needs. Not surprisingly, many of the programs considered innovative in public health today, such as screening for chronic diseases, for example, are addressed toward meeting unmet health needs traditionally allocated to private medicine. The public health ideology instrument was therefore designed to determine how far the health officer leaned toward either of these poles by asking his opinion of the proper locus of control for twenty-six kinds of medical and health care. The form and method of administration of this instrument were essentially the same as that of the activism scale.

Turning now to the findings concerning attitudes of leaders, our first concern is with the relationship between activism and ideology themselves. If it could be shown that each measure was correlated with innovation, but not highly intercorrelated, then they could profitably be combined. Their joint explanatory power would be greater than either attitude alone. Our day-to-day experience indicates that the liberal is not necessarily energetic in pursuing his views, nor is the conservative necessarily a passive reactor to events. There was good reason to predict that activism and ideology would not be strongly related and that indeed proved to be true with this group of local health officers. The correlation between the two variables is $r = .27$, a statistically significant relationship in a group this size ($p < .005$), but certainly not a strong one.[26] For further analyses, the single variable "activism-ideology" was constructed, which represents the simple sum of the two attitude scores for each health officer.[27]

The finding of central interest in this section, the correlation between the activism-ideology of the health officer and progressive programming, is $r = .36$. However, to interpret that finding it is necessary to rule out the effects of community size, since progressive programming is an expression of the absolute amount of resources added in non-traditional program areas during 1960–1964, and large communities will naturally be served by greater numbers of personnel than small ones. When the control is introduced, the resulting partial correlation becomes $r = .40$,[28] indicating that the size of communities makes essentially no difference for the relationship between activism-ideology and progressive programming in these agencies.[29]

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[26] A significance level does not have the usual meaning here, since the group studied is a population rather than a probability sample. However, the test does provide some additional information about the strength of the reported relationship. It tells us the probability that a relationship this strong would appear if the group were divided into categories at random rather than according to actual scores on the independent variable. In light of these considerations, I have elected not to sprinkle significance levels throughout the report but to provide some bench marks that may be used as a guide by the interested reader: For $N = 93$, using a one-tailed test and the .05 level, the correlation $r = .18$ is significant; the comparable coefficients for other sample sizes in which we will be interested are $r = .21$ (for $N = 69$), $r = .24$ (for $N = 49$), and $r = .30$ for ($N = 33$).

[27] Since the standard deviation of the ideology scores is greater than that of the activism scores (10.9 to 7.8), ideology contributes slightly more than activism to the summed index.

The relationships just reported do not hold uniformly for the entire group of health officers studied. In Ontario, apparently owing to cultural differences in attitude toward the role of government and the station of private medicine, the relationship between ideology and innovation was slightly negative.[30] The data do not provide a direct explanation of this finding, but it became quite evident during the interviews that many Ontario health officers conduct a number of innovative programs in earnest while at the same time believing that this type of activity is, in general, more properly the responsibility of private medicine. From responses to open-ended questions, it appears that Ontario health officers generally feel even more strongly than those in the four American states that personal health matters are the province of private practice, but they also feel it quite proper for the government to step in when this responsibility is not being fully met. The orientation is a peculiar but understandable mixture of British socialism on one hand and small town American capitalism on the other. Because of such differences, ideology as measured here was not a good indicator in Ontario of the motivation to innovate, nor was it in any other way a good predictor of innovation in the Ontario departments. With this understood, the findings reported below will at times be given both for the group as a whole and also for the four American states considered separately ($N = 69$). In the American states alone, for example, the correlation between progressive programming and the activism-ideology of the health officer is $r = .40$, and when community size is controlled the partial correlation becomes $r = .44$.

It is desirable to show how activism-ideology is related to the total number of non-traditional programs adopted as well as to progressive programming; only then may we really compare these results with the results of other studies, most of which have defined innovation as the simple adoption of new ideas. However, the total group of organizations cannot be used in this analysis; many programs might have been adopted before the incumbency of the current health officer, making his attitudes largely irrelevant.[31] We must use a subgroup of organizations in which all adoption of non-traditional services can reasonably be attributed to the health officer who was interviewed. Specifically, the subgroup includes all health officers who had held the post in that organization for at least 15 years, i.e., since 1950, plus each health officer whose annual rate of adoption during 1960–1964, when multiplied by his total years of tenure, yields a product that is equal to or greater than the total number of innovative programs adopted by the department.[32] This group contains 33 U. S. organizations and, including Ontario, 49 departments in all. The subgroup is small but quite well representative of the entire study population on key indicators.[33] We find that in this subgroup the correlations between activism-ideology and adoption are essentially the same as those reported above, when progressive programming was the dependent variable. In the 33 U. S. departments in the subgroup, for example, the uncontrolled correlation between activism-ideology and the number of programs adopted is $r = .38$. When community size is controlled, the partial correlation becomes $r = .43$. In the entire subgroup of 49 organizations, the corresponding product-moment correlations are $r = .38$ and $r = .40$. (To make comparisons among groups and subgroups convenient for the reader, separate intercorrelation matrices among the major variables in the study are provided in Tables 1 and 2).

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[28] Separately, the correlation between activism and innovation, controlling for community size, is partial $r = .32$; for ideology and innovation, controlling for size, partial $r = .29$.

[29] It will be of interest to note here how much innovation actually took place. Progressive programming ranged in these agencies from zero to 25.5 man-years, with a mean of 3.5 and a standard deviation of 4.9. Adoption, which will be considered in a moment, ranged from 2 programs to 27, with a mean of 11.7 and a standard deviation of 6.1.

[30] $r = -.17$, a non-significant correlation.

[31] Nor would it have been feasible to consider only post-1960 adoptions: such a procedure would have penalized the early adopters, many of whose innovations were made prior to 1960.

[32] For example, one health officer had adopted 1 non-traditional program per year during 1960–1964 and had been in the job for 10 years. One might, therefore, reasonably estimate that 10 of the department's non-traditional programs are associated with his incumbency. Since this department had a total of 14 non-traditional programs it was not included in the subgroup. If its total had been 10 or less, it would have been included. If the health officer had been there for 15 years, the department would have been included regardless of total number of adoptions, for almost none of these programs had been introduced in local health departments before 1950.

[33] The means for the whole group and for the subgroup, respectively, are: number of programs adopted, 11.7 and 11.2; population of jurisdiction, 110,000 and 109,000; 1959 expenditures, $188,000 and $165,000; activism-ideology, 119.9 and 120.1.

Thus, we have good reason to conclude that the motivation of the health officer, as measured, is indeed related to the innovation of the department, whether this be defined as number of programs adopted or as increments in resources devoted to those programs.

On the other hand, it is noteworthy that behavior in this study is only *moderately* predictable from attitudes, and especially from ideology. Furthermore, the zero order correlation between ideology and innovation observed here ($r = .24$, $N = 93$) is not substantially different from that obtained in similar studies. In the research of Fliegel and of Rogers cited above, the comparable relationships were $r = .42$ and $r = .26$, respectively. In the study by Hage and Aiken, the comparable measure was slightly negative ($r = -.15$). Blau and Mytinger concluded that ideology was important for change, but their results were not obtained through systematic measurement of the ideology of respondents and subsequent statistical analysis of all cases.

While it is possible that ideologies are so difficult to measure that their true effects cannot often be captured, two alternative explanations seem more compelling. Both begin with the inference, drawn from the empirical results discussed above, that ideology *in itself* is indeed *not* an important predictor of innovation. The first alternative is that a mere willingness to innovate, as reflected by ideology, is an attitude of secondary importance to the *determination* to innovate. The second alternative is that an attitude such as ideology may lead to predicted behavior only when *other* important conditions are also favorable. These possibilities suggest two research strategies. One is to measure the intensity with which an ideology is held in addition to measuring its mere existence, for in that way one may well capture the difference between those who are merely willing to behave consistent with an ideology and those strongly impelled to do so. Secondly, there should be an attempt to measure other conditions that may determine whether or not an ideology will be expressed in behavior in a given situation and to

TABLE 1. CORRELATIONS AMONG THE PRIMARY VARIABLES FOR ALL HEALTH DEPARTMENTS STUDIED*

	B	1	2	3	4	5
A. Adoption	*.70* .70	*NA* NA	*.40* .46	*.67* .65	*.64* .63	*NA* NA
B. Progressive Programming		*.40* .36	*.33* .41	*.60* .60	*.60* .61	*.49* .33
1. Health officer activism-ideology			*−.04* .01	*.10* .12	*.08* .10	*.24* .15
2. % of the population in white collar occupations				*.41* .42	*.41* .45	*.28* .11
3. Health department expenditures, 1959					*.88* .88	*.35* .26
4. Population of the health jurisdiction, 1959						*.38* .26
5. Extent of public health training of key employees						

* Correlations in italics apply to the U.S. health departments studied, only.

NA: Not applicable, since some adoptions may have predated the incumbency of the health officer and key employees.

TABLE 2. CORRELATIONS AMONG THE PRIMARY VARIABLES FOR HEALTH DEPARTMENTS IN WHICH ADOPTION IS ASSOCIATED WITH THE HEALTH OFFICER INTERVIEWED*

	1	2	3	4
A. Adoption	*.38* .38	*.30* .36	*.70* .63	*.64* .63
1. Health officer activism-ideology		*−.21* −.14	*.10* .12	*.08* .10
2. % of the population in white collar occupations			*.27* .30	*.23* .30
3. Health department expenditures, 1959				*.83* .83
4. Population of the health jurisdiction, 1959				

* Correlations in italics apply to the U.S. health departments studied, only.

analyze the relationship between attitudes and behavior in the light of such conditions. In the present study, for example, it is quite possible that health officer attitudes resulted in innovative behavior only when other conditions, such as the balance between obstacles and resources, were also favorable. This possibility will be explored after other relevant findings have been presented.

III. OBSTACLES

Two sources of obstacles were thought to be important in explaining differences in innovation among local health departments: the extent to which a) the community and b) the organization itself are prepared to accept or resist such departures from established practice.

Community obstacles to innovation. Since no direct measure of community readiness to innovate was available, an attempt was made to study it indirectly by ascertaining social class levels, as measured by education (per cent of the population 25 years old and older who have completed high school) and occupation (per cent of the labor force in white collar occupations). The rationale for using these data as measures of readiness is derived from other research. It has been shown that the social class of an individual is correlated with his beliefs and behavior regarding the efficacy of preventive health actions;[34] by extension to the aggregate level, public acceptance of the efficacy of preventive health measures may render a community favorable to action by its local health department. Another type of evidence indicates that the higher social classes tend to vote in favor of measures (such as bond issues) perceived as benefitting the general public, even to the extent of voting against their own apparent self-interest. Banfield and Wilson refer to this attitude as "public-regardingness."[35]

Our finding is that education level, as measured, is only weakly related to innovation (the correlation with progressive programming is $r = .23$; with adoption, $r = .24$). When controlled for community size, the partial correlation of education level with progressive programming is only $r = .03$, and with adoption, $r = .04$.[36] Occupation level, on the other hand, was strongly enough related to innovation to retain at least some independent explanatory power. The extent of white collar employment is related to the number of programs adopted at the level $r = .46$ and to progressive programming at the level $r = .41$. When community size is controlled the relationships drop to $r = .25$ and $r = .19$, respectively, so that occupation level does not provide a great deal of additional explanatory power once the size of the community is considered. We will keep in mind, however, that if extent of innovation seems to be determined by community size the reason may lie partly in a relationship between size and social class characteristics. At some level of probability, for example, the larger the health jurisdiction, the greater the per cent white collar, and therefore the greater the readiness to accept innovations in public programs.

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[34] See Don P. Haefner, S. Stephen Kegeles, John P. Kirscht, and Irwin M. Rosenstock, "Preventive Actions Concerning Dental Disease, Tuberculosis, and Cancer," *Public Health Reports,* 82 (May, 1967), 451–459.

[35] James Q. Wilson and Edward C. Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," this REVIEW 58 (December, 1964), 876–887.

[36] The percent who completed two or more years of college may well have been a better measure for our purpose. Unfortunately, such information is not available from the normal central sources, such as the *County and City Data Book* published by the U.S. Bureau of the Census. It should definitely not be concluded on the basis of these correlations that aggregate education level has little or nothing to do with resistance to change in communities.

Organizational obstacles to innovation. Within the organization, the readiness to accept innovations was measured by the extent of public health training of key lower echelon employees—their capacity to handle the supervisory aspects of the new programs.[37] Some of these supervisors hold advanced degrees in public health, some have had undergraduate programs with substantial public health content, and some have participated in special courses administered by state and federal health agencies and by nearby universities. Some, on the other hand, have had little or no specific public health training; their supervisory capacity is based largely on years of work experience with traditional programs.

A correlation of $r = .33$ was obtained between the public health training of supervisors and progressive programming in the department. When controlled for community size, the partial correlation becomes $r = .22$. It is of interest to note that the lack of training of supervisors is an obstacle primarily when the *health officer* is motivated to innovate. Dividing the organizations into two approximately equal groups according to the activism-ideology of the health officer, we find that when motivation to innovate (as measured) is low, the correlation between progressive programming and the training of supervisors is only $r = .15$, but when motivation is high, this relationship is just under $r = .40$. Thus, when the health officer appears ready to innovate, lack of training at the supervisory echelon can apparently constitute one further obstacle to organizational innovation.

The interview data, supported by impressions obtained during the field work, suggest that continuing education of staff nurses and sanitarians through short courses dealing with the more novel program areas is effective in winning their cooperation and enthusiasm. When asked whether the department operated a specific program, many health officers complained that the unwillingness of lower echelon employees had proven to be an insurmountable obstruction to desired innovation. This seemed to apply particularly to nurses quite advanced in age, a fairly common personnel characteristic of local health departments. On the other hand, new programs such as mental health nursing were frequently established or substantially expanded in some departments after most or all of the employees to be involved had taken the opportunity to receive special training for these new duties. There was no evidence of an employee's refusing or even resisting such training. Thus, if they are not already doing so, it is suggested that leaders who would like to put more emphasis on non-traditional services should devote serious attention to the matter of employee training.

IV. RESOURCES, SIZE AND INNOVATION

It was expected on the basis of prior research that a great deal of the variance in program innovation among local health departments might be explained by the amount of money available to the health department for innovation. The primary operational definition of resources, therefore, was simply the level of expenditures of the health department in 1959 (the last year prior to the period over which progressive programming was measured). Clearly, expenditures do not precisely indicate resources available for innovation, since funds might possibly be inextricably bound to ongoing, traditional activities. It was tentatively assumed, however, that greater absolute income would generally imply greater discretion in the commitment of resources, not only in local health departments but also in organizations in general. For example, the health department with ten nurses might send one nurse at a time to a training course in mental health, but the health department with only one or two nurses might have great difficulty in doing so.

The finding is that there is indeed a strong relationship between expenditures and innovation in these organizations—stronger than that between innovation and either the activism-ideology of the health officer or obstacles presented by community and organizational factors. The correlation between expenditures and progressive programming is $r = .60$, and that between expenditures and the number of non-traditional programs adopted is $r = .65$. We must ask, however, whether "expenditures" is really an indicator of resources or, perhaps, an indicator of the motivation to innovate. Is there a strong relationship because larger *communities* need or demand more programs, or because the greater resources accompanying *organizational* size *enable* more extensive innovation in some departments?

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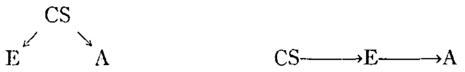
[37] Extent of public health training was measured on a ten-point scale. The data were obtained through a self-administered questionnaire left with the supervisors at the time of the health-officer interview. It would have been far better to have obtained this information on all professional employees in the department, but turnover is such that very few in each department had been there from 1960 through 1964.

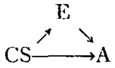
One excellent way to attack this question is to carry out a three-variable causal analysis using partial correlation and regression.

Unfortunately, extreme multicollinearity would seem to render current methodology for the evaluation of causal models unreliable in this case.[38] Despite this problem, and especially considering that there is no attempt here to make inferences to a sampled universe, the analysis was undertaken for the insights and cues to further analysis it might provide.

The partial correlation between expenditures and adoption, controlled for community size, is $r = .27$. In terms of causal interpretation, this result is ambiguous. The relationship is depicted by model *c* in Figure 1, but one wonders whether it actually looks *more* like model *a* or model *b*, both of which are contained in model *c*. Since essentially the same coefficient is expected in both cases, the partial correlation does not enable us to ascertain whether this is *primarily* a relation of spuriousness (expenditures and adoption are related mainly by the coincidence that community size caused them both to vary) or a developmental sequence, community size caused expenditures to vary and expenditures, in turn, caused adoption to vary). However, the partial slope of adoption on expenditures, controlling for community size, does suggest a particular causal interpretation. If the true causal relationship were developmental (model *b*), a control for community size would leave the partial regression coefficient essentially unchanged—the slope should not be affected by controlling for a prior cause—whereas if complete spuriousness were involved (model *a*), the partial regression coefficient would be reduced to zero.[39] Analysis shows that in this case, the slope is in fact reduced, but not substantially, when controlled for community size. The zero-order slope of adoption on expenditures is $b = .017$, and when controlled for size the partial slope becomes $b = .011$. The inference to be drawn is that a partially spurious relationship exists, but one that includes a substantial direct effect of expenditures on innovation. Community size causes organizations to be large (while at the same time motivating them to be innovative in some small degree) and the size of the organization then makes possible the variety of programming that constitutes the dependent variable.



a. Spuriousness b. Developmental Sequence



c. Partial Spuriousness

FIG. 1. Possible causal relationships among health department expenditures (E) the adoption of innovative programs (A) and community size (CS).

Let us now examine some additional factors in order to place size in even sharper perspective. In preceding analyses, it was observed that many variables associated with innovation were also associated with community size, suggesting that the effects of size might not be so pronounced if these other specific variables were not involved. These include the activism of the health officer, the training of key employees, and, most importantly, the expenditure level of the health department. Although the correlation between community size and the number of innovative programs adopted is $r = .63$, when it is controlled for health department expenditures, the relationship is reduced to partial $r = .15$. When additional controls are introduced for health officer activism and per cent white collar, the relationship is reduced further to partial $r = .09$. This "partialling out" of the effects of community size implies that, for these data, how community size acted upon innovation in the health department may be almost completely understood in terms of the other variables just listed.

Thus, we can tentatively conclude that community size was important for innovation in this study because it connoted a summary of factors that included motivation, obstacles, and resources in a highly conducive combination. Neither community size, farm size, size of health department, nor size of firm should be accepted hastily as an accurate predictor of innovation. Size itself is not related to innovativeness by logical necessity; it becomes significant only when it implies or indicates the conceptual variables that are important in themselves.

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[38] The correlation between community size and health department expenditures is $r = .88$; between expenditures and adoptions, $r = .65$; between size and adoption, $r = .63$.

[39] See Hubert M. Blalock, Jr., *Causal Inference in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1961), pp. 83–91.

Motivation, obstacles, and especially resources account substantially for differences in the number of innovative programs operated by agencies of different size. It is now desirable to ascertain whether larger health departments put greater relative emphasis on innovative services than smaller ones, or if they merely adopted many more programs. A new dependent variable was constructed to investigate this question—the proportion of total increases in resources devoted to initiating and expanding non-traditional services during the period 1960–1964, or, put another way, the ratio of progressive programming to increases in all programs, new and existing, traditional and non-traditional. For brevity, let us call this variable "proportional innovation." The correlation between health department expenditures and innovation so defined is $r = .04$. This result is surprising, but unequivocal; health department size and, presumably, the resources available as a consequence of size, had on the average almost no impact upon proportional innovation during this period of time.

It would be incorrect to infer that the resources bound up with organizational size (or even only with local health department size) are invariably unrelated to proportional innovation. It may be, for example, that had this same study been carried out ten years earlier, the correlation between health department expenditures and proportional innovation would have proved substantially higher. It is possible that at that time very few small departments had adopted any non-traditional programs, whereas the large departments no doubt already had. In that case, the correlation between expenditures and proportional innovation would almost certainly have been greater ten years ago than .04, although the total amount of innovation in all health departments at that time may have been quite small. The fact remains, however, that size—and therefore the resources implied by size—was not associated with greater proportional innovation in these agencies. Until contrary empirical evidence and sound theoretical justification are presented, one must at least remain skeptical of a causal relationship between organizational size and the proportion of increased resources that will be allocated to innovation.

Proportional innovation might perhaps be considered a faulty operational measure in that large departments, because of a substantially greater growth rate, may actually be more innovative than small departments when they devote approximately the same proportion of their growth to non-traditional services. Growth rates are not greater with increased size in these organizations, however, nor has such been found to be true of other kinds of organizations. "It is now generally agreed that there is little or no relation between size of firm and expected percentage rate of growth. Mansfield (pp. 1034–35) finds slightly higher growth rates for small firms *that survive* than for large firms, but the differences are slight, and do not affect the general argument."[40] The correlation between health department size and growth during 1960–1964 as a percentage of size is $r = -.16$. Thus, large and small organizations are increasing at roughly the same rate.

Although it is clear that size on the average had no appreciable impact on proportional innovation, it is equally clear that large departments did tend to adopt substantially more non-traditional programs than small ones and it is germane to ask which resources may have led to this effect. To find answers to this question, an analysis was made of the median size of the health departments adopting each non-traditional program. Each program was assigned a numerical score—the median size of the agencies that adopted it—and the highest and lowest one-third of a list of programs ranked according to this criterion were singled out for special attention. The complete analysis is too lengthy to be presented here in detail, but the major conclusions can be stated briefly. First, the need or demand for a greater variety of programs in large communities occasioned only a few of the observed differences in number of programs adopted. Services such as air pollution control, the coordination of a number of community health agencies, and occupational or industrial health were, as might be expected, more likely to be found in large urban health jurisdictions. The remaining differences suggested by the two lists may be accounted for most reasonably by differences in resources between small and large departments. The effective resources appear to be as follows:

a. Small departments reach an early limit on variety of programming because of a limitation on the number of different activities into which one person's time may be fragmented. Large departments are not so limited simply because their greater number of personnel gives them the flexibility of assigning at least one full-time or half-time employee to each of a great many services.

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[40] Herbert A. Simon, "Comment: Firm Size and Rate of Growth," *Journal of Political Economy*, 72 (February, 1964), p. 81. See also the articles by Mansfield and by Simon and Bonini cited there.

b. Small departments have difficulty attracting specialized personnel such as nutritionists, research scientists, social workers, and medical specialists. This limitation tends to restrict their non-traditional programs to those for which traditional kinds of personnel may be employed. Large departments attract specialized personnel much more easily.

c. Small departments have little slack in funds obtained from local sources, thereby depending heavily on single-purpose grants for innovation. Large departments, on the other hand, are able to devote a substantial percentage of their local funds to non-traditional programs.

d. Small departments innovate largely in program areas for which grant funds are readily available from the state health department. Large departments are more likely to receive funds and personnel directly from the federal government, as well.

These, then, appear to be the important specific resources enabling large local health departments to adopt many more non-traditional programs than smaller ones. Note that in another setting, such as an industrial organization, the effects of size upon innovation would no doubt be due to an entirely different set of resources, and in some general cases, size may have no implications whatever for the resources available for innovation.

Because they lack the resources needed for diversification, small health departments are unable to support a great variety of non-traditional services. However, by intra-organizational transfers, state grants and other means, they were able to find the resources to place as many personnel in non-traditional programs as larger agencies, in proportion to size, over the five-year period, 1960–1964. Moreover, when they did acquire additional resources during this period, they put as great a proportion of the total into non-traditional programs as did their larger counterparts. These three facts lead one to suspect that, on the average, the smaller agencies devoted their increased resources to relatively fewer non-traditional programs but on a relatively greater scale. A case-by-case analysis confirmed this suspicion. On the other hand, large health departments are able to support a great variety of services and, clearly, do so. They are exercising an option, however. Why should the large departments choose to adopt a large number of programs when they could easily use the same funds to launch fewer programs on a larger scale or to expand existing innovative enterprises?

This question may be answered at least in part by the concept of "slack" innovation introduced by Cyert and March.[41] Slack innovation in these organizations would be innovation motivated largely by a desire for prestige and professional status on the part of the health officer and other health department staff members. In general, if organizational capability remains after the most pressing problems are attended to, some or all of such capability tends to be allocated to status-motivated innovation. Note that this outcome would probably be manifested here by a greater number of different non-traditional activities, as opposed to increased emphasis upon existing activities, which does not ordinarily confer further prestige. Thus, since variety brings the bonus of professional approval, and since large departments have the resources to support greater variety, they have a strong tendency to adopt many more innovative programs than their smaller counterparts. In the general case, when an organization possesses the resources necessary to operate more innovative programs and can thereby obtain professional approval for its members, we would expect to find that it does indeed operate more. One important contributory condition is the general absence of systematic, dependable, retrospective evaluation of decisions regarding the allocation of resources.

Thus, this discussion points to large organizational size as a *facilitator* of innovation more than as a motivator of innovation. The analysis is consistent with the hypothesis concerning slack innovation offered by Cyert and March and it also helps explain some apparent discrepancies in the relationship between size and innovation in industrial firms. For small firms, technological advances may often be examples of what Cyert and March call "problem" innovation, since the firm may need these new techniques to bring about indispensible improvements in its market position. For this reason, relatively unsuccessful firms have been found to be leaders in the adoption of some specific, extremely important technological improvements.[42] However, if one studies a broad range of innovations, the larger and more successful firms—the firms more able to undertake slack innovation—should be found to lead the field in total adoptions. Such was the finding of Mansfield in his research,[43] and that finding has been replicated here with respect to local health organizations.

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[41] Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, New Jersey: Prentice-Hall, 1963), pp. 278–279.

[42] Walter Adams and Joel B. Dirlam, "Big Steel, Invention, and Innovation," *Quarterly Journal of Economics*, 80 (May, 1966), 167–189.

It may by no means be concluded from this discussion that either large or small departments do a more effective public health job—because of greater breadth of innovative programming, on one hand, or greater depth, on the other. One can think of numerous reasons why one style or the other might lead to a more effective total program, or why style in this sense would be totally irrelevant. In the last analysis, the question can only be resolved by systematic evaluation of total program effectiveness.[44]

Nor should we conclude that innovation is necessarily frivolous when motivated by concern for professional status rather than organizational production or service goals. A more realistic view is simply that such innovation is often non-rational: how adequately a formal organization will serve the needs of its public, especially unexpressed needs, appears to depend largely upon the values assumed by one or more professional groups in their own struggles for social prestige and survival. It is problematic whether value choices that are good or bad for a professional group lead to action choices that are good or bad for an organization and its public. No doubt, status innovation is sometimes at the root of profound progress while at other times it is merely a waste of resources.

V. A MULTIPLICATIVE MODEL OF INNOVATION

Support has been provided for the inference that innovation is a function of motivation, obstacles, and resources. It remains to determine how these variables act in combination. In such a case, the political scientist and the sociologist frequently turn to the linear additive multiple correlation procedure, a mathematical model that suits the exigencies of survey data quite well. If the analyst were to use a multiplicative model, for example, he might well find himself in a series of awkward positions as he confronted negative values, the desirability of making changes of scale (such as standardizing), and the absence of true ratio scales, as in attitude measurement. The tools for depicting non-additive relationships mathematically are not as accessible, nor do they appear to be as versatile as additive tools.

Multiple correlations provide quite "good" results, in terms of proportion of variance explained, when applied to the data of this study. (For example, the multiple correlation of progressive programming with expenditures, activism-ideology, per cent white collar and supervisor training in the four American states is $R = .73$.) However, it is not entirely reasonable to apply such a model. It is more reasonable to expect an interactive rather than an additive result.

One relationship of interest is that between the activism-ideology of the local health officer and the adoption of non-traditional programs by the department. In the group of forty-nine health departments in which the health officer interviewed was highly likely to have presided over the initiation of all the agency's non-traditional programs, it was found that these two variables were related at the level $r = .38$, both in the study population as a whole and in the U. S. health departments considered separately. It may well be, however, that this relationship is contingent to some extent upon the balance of obstacles and resources prevailing in different cases. When the obstacles are relatively great and the resources small, for example, it would be predicted that even the highly liberal and active health officer might not find it possible to innovate, at least not in substantially greater measure than his counterpart with relatively low motivation. If the balance were tipped heavily in the other direction, however, health officer motivation would be expected to make a great deal of difference. The highly motivated health officer would probably be extremely innovative, while his counterpart with relatively little motivation might not introduce any changes at all in spite of the ease with which he could apparently do so.

In order to test this prediction the group of forty-nine health departments was divided into "high" and "low" subgroups according to the level of resources available to them, and the regression of innovation on health officer attitudes was examined within each subgroup. A relatively low regression coefficient would be expected in the group characterized by few resources, but a relatively high slope in the group with resources more favorable to innovation, where the attitudes of the health officer would be likely to have substantial effect.

In the total group of forty-nine organizations, the regression coefficient of innovation (the adoption of non-traditional programs) on the activism-ideology of the health officer is $b = .15$. In the twenty health departments whose expenditures in 1959 were below $100,000, the comparable slope was found to be $b = .06$, whereas in the twenty-nine health departments with expenditures above $100,000, it is $b = .28$. When resources are high, in other words, a unit increase in health officer motivation, as measured, has about 4½ times the effect upon innovation as it does when resources are low. (In the U. S. health departments considered separately, the comparable coefficients are $b = .16$, for the group as a whole, and $b = .07$ and $b = .22$ for the subgroups characterized by low and high resources, respectively.) This difference in slopes suggests that there is, in fact, an interaction in the relationship such that the two independent variables, motivation and resources, may well provide an accurate *multiplicative* prediction of innovation.

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[43] Mansfield, *op. cit.* See also his, "Technical Change and the Rate of Imitation," *Econometrica*, 29 (1961), 741–766.

[44] For an excellent methodology for evaluation of public programs, see O. L. Deniston, I. M. Rosenstock and V. A. Getting, "Evaluation of Program Effectiveness," *Public Health Reports*, 83 (April, 1968), 323–335.

Since we are dealing here with three interval—or assumed interval—scale variables, it is possible to go somewhat beyond the recognition of a difference in slopes in testing the fit of these data to a multiplicative mathematical model. The model would state that

$$I = k \cdot M^{b_1} \cdot R^{b_2}, \qquad (1)$$

where I = Innovation, M = Motivation, R = Resources, and k is a constant of proportionality. However, we may establish a prediction equation using the transformation

$$\log I = \log k + b_1 (\log M) + b_2 (\log R) \qquad (2)$$

and use standard least-squares procedures for the purpose of estimation.[45] It would then be of interest to calculate the goodness-of-fit of this logarithmic expression of the relationship, and to compare it with the fit of an ordinary linear additive model to the same data, employing the same variables. The ordinary multiple correlation of the adoption of non-traditional services with both health officer attitudes and health department expenditures, using only this subgroup of forty-nine health departments, is $R = .70$. In terms of proportion of variation explained, $R^2 = .49$. The proportion of variation in these same innovation scores explained by the antilog of the right hand side of equation (2), above, is $r^2 = .53$. Thus, the multiplicative model provides at least as good a fit to the data as the additive model. When only the health departments in the four American states are considered, the goodness-of-fit of the additive model is $R^2 = .59$, whereas the multiplicative model again yields a slightly higher goodness-of-fit measure, $r^2 = .66$ (as a correlation, $r = .81$). When obstacles are considered as well,[46] the goodness-of-fit measures for the multiplicative model in the whole group and in the U. S. only are $r^2 = .63$ and $r^2 = .72$, respectively, whereas this time for the additive model they are exactly the same, $R^2 = .63$ and $R^2 = .72$.

Thus, the empirical observations are consistent with the hypothesis that innovation is a multiplicative function of the motivation to innovate and the resources available for innovation. We were not able to obtain a highly satisfying discrimination between the two models; it is true that the multiplicative model does not provide a clearly better fit than the additive model, or *vice versa*. However, for two reasons noted by Blalock, fairly good additive approximations of relationships that are actually more complex are to be expected: "In general, additive models seem to approximate reality reasonably well, given a limited range of variation and sizeable 'random' fluctuation that make accurate specifications difficult."[47] As behavioral science progresses, however, we will hopefully capture a greater range of values on each variable employed, so that the nature of effects will be more clearly delineated, and we will also both improve the validity of specific measurements and consider or control more of the factors relevant to each analysis, so that random and nonrandom fluctuation will be reduced. As we approach attainment of these objectives, it is logical that the mathematical models we employ be dictated as much as possible by theoretical considerations and as little as possible by disciplinary tradition. When two fairly simple forms of explanation are equally effective in organizing empirical events, the one that is theoretically more cogent is to be preferred.

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[45] Blalock, *Causal Inferences in Nonexperimental Research, op. cit.,* pp. 91–92. For other suggested treatments of multiplicative relationships, see Hubert M. Blalock, Jr., "Theory Building and the Statistical Concept of Interaction," *American Sociological Review,* 30 (June, 1965), 374–380; James S. Coleman, *Introduction to Mathematical Sociology* (New York: The Free Press of Glencoe, 1964), pp. 224–235; Bruce M. Russett, Hayward R. Alker, Jr., Karl W. Deutsch, and Harold D. Lasswell, *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964), pp. 322-340; Hayward R. Alker, Jr., *Mathematics and Politics* (New York: The Macmillan Company, 1965), pp. 108–111; Hayward R. Alker, Jr., "The Long Road to International Relations Theory: Problems of Statistical Nonadditivity," *World Politics,* 18, (July, 1966), 639–644.

[46] The resources term in the equation becomes, in this case, an obstacles-resources term. It was calculated by adding together the expenditures and percent white collar scores, after first weighting each with the appropriate partial slope from the regression of adoption on activism-ideology, expenditures, and percent white collar.

[47] Blalock, "Theory Building and the Statistical Concept of Interaction," *op. cit.,* p. 375.

It has been suggested more than once that the kind of theoretical case confronted in the present research involves a multiplicative relationship. Atkinson has proposed a general theory to account for "motivated" behavior, or achievement-oriented behavior, in which any such behavior is a multiplicative function of motivational variables, on one hand, and "expectancy" (the subjective probability of success), on the other.[48] Note that this latter psychological variable may easily be translated into our terms as an objective probability of success—the balance between resources and obstacles. The present results, therefore, may be seen as a specific instance of the operation of Atkinson's hypothesis. In yet another study, Palmore and Hammond have come to an identical analytical conclusion regarding closely similar independent and dependent variables.[49] After analyzing records of welfare recipients in New Haven, these investigators concluded that deviant behavior (juvenile delinquency) is a multiplicative function of what may again be viewed as indicators of motivation and resources. The motivational variables are summarized by the concept "blockage of legitimate opportunity" and are indicated operationally by race, sex, and success in school. The resource-obstacle variables are summarized by the concept "availability of illegitimate opportunity," and are indicated by measures of both family deviance and neighborhood deviance. Blalock, in fact, has attempted to draw a direct analogy between the variables discussed by Palmore and Hammond and those discussed by Atkinson.[50]

Taking Atkinson's proposed theory of "motivated" behavior as a hypothesis, we note that Palmore and Hammond have used the same kind of model and what appear to be very similar conceptual predictors to explain *deviant* behavior. The present research has again used a multiplicative model and similar independent variables to explain *innovative* behavior. Together, these two studies provide substantial empirical support for Atkinson's more general psychological formulation. Moreover, the concepts of innovation and social deviance are closely allied. Merton has specifically designated the deviant behavior of the gangster and the juvenile delinquent as "innovation."[51] In his well-known paradigm, innovation is distinguished from other modes of adaptation to cultural norms—conformity, ritualism, retreatism, and rebellion—by constituting a behavioral pattern in which cultural goals are accepted but institutionalized means for achieving them are rejected. This formulation has been clarified by Harary and others, so that the meaning of "the rejection of institutionalized means" in the case of innovation is expanded to include the substitution of other means for attaining the culturally approved goal.[52] The delinquent is an innovator, according to Merton, in that he has accepted the cultural goal of individual material success but rejected socially accepted means, and adopted non-accepted means, of attaining that objective.

Thus, it has been suggested that several related forms of behavior depend multiplicatively on the motivation to venture into a new pattern of action and the net resources available for accomplishing that purpose. The hypothesis has grown out of the analysis of empirical observations as well as from more abstract inference. Further research is now needed to elaborate upon this theoretical proposition, which would seem to have exceptionally wide application in the social sciences.

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[48] John W. Atkinson, "Motivational Determinants of Risk-Taking Behavior," *Psychological Review*, 64 (1957), 359–372. Atkinson actually uses two aspects of total inclination to act—basic psychological drive, such as the need for achievement, and incentive value attached to the object of the behavior in question. These are multiplied together and it is their product that is multiplied by the expectancy factor to obtain a prediction of behavior. The present study does not distinguish the basic drive from the incentive component of overall motivation; both are probably involved to a certain extent in motivation as measured here. Atkinson's article has most recently been reprinted (and also updated) in John W. Atkinson and Norman T. Feather (eds.), *A Theory of Achievement Motivation* (New York: John Wiley, 1966), Chapter I. See also Chapter II, "Notes Concerning the Generality of the Theory of Achievement Motivation," by Atkinson and Chapter 20, "Review and Appraisal," by Atkinson and Feather.

[49] Erdman B. Palmore and Phillip E. Hammond, "Interacting Factors in Juvenile Delinquency," *American Sociological Review*, 29 (December, 1964), 848–854.

[50] Blalock, "Theory Building and the Statistical Concept of Interaction," *op. cit.*, p. 379.

[51] Merton, *op cit.*, pp. 131–194. See especially, p. 140.

[52] Frank Harary, "Merton Revisited: A New Classification for Deviant Behavior," *American Sociological Review*, 31 (October, 1966), 693–697.

VI. CONCLUSIONS

This research began with a concern for the responsiveness of public agencies to changes in the general problems they were designed to meet. We did not study the whole of this question but rather attacked one essential facet, the ability of such organizations to adopt and emphasize programs that depart from traditional behavior. On the other hand, our focus goes somewhat beyond the problem of responsiveness in that it is relevant to questions of organizational innovation in general, whether justified by environmental changes or prompted by any of a host of other possible stimuli.

It was initially proposed that extent of innovation is a negative function of obstacles and a positive function of the motivation to innovate and the availability of relevant resources. The data collected from 93 local public health organizations supported this proposition, although, as operationalized, the relationships were weak in some cases. The variable emerging as by far the most powerful predictor of innovation was "size," but we concluded that this relationship cannot be considered theoretically complete. Rather, size should be expected to predict innovativeness only insofar as it implies the presence of motivation, obstacles and resources. In the present case, organizational size proved to be an excellent indicator of the relevant resources available to local health departments for the adoption of a large number of non-traditional programs.

The analysis of the data yielded several additional conclusions that would seem to merit further test and elaboration:

a. In attempting to derive accurate predictions of behavior from attitudes or beliefs, such as ideologies, the belief itself should be qualified by the intensity with which it is held and one may well expect the belief-behavior relationship to be qualified by one or more contingent conditions.

b. With regard to many kinds of innovation in public organizations, the relationships among community size, organizational size and extent of innovation form a developmental sequence. Larger communities generally require larger service organizations, and it is the resources of the larger organization that lead to innovation. Thus, large organizations in small communities may be expected to adopt many innovations: small organizations in large communities will adopt few.

c. A great deal of innovation in organizations, especially large or successful ones, is "slack" innovation. After solution of immediate problems, the quest for prestige rather than the quest for organizational effectiveness or corporate profit motivates the adoption of most new programs and technologies.

d. Once the diffusion of innovations has progressed far enough to include late adopters, organizational size and wealth will have no bearing upon the relative emphasis accorded to innovative as opposed to traditional activities.

e. Lastly, and perhaps most importantly for theoretical development, innovation is viewed as a multiplicative function of the motivation to innovate and the balance between the obstacles and resources bearing upon innovation.

INDICATORS OF CROSS-NATIONAL AND INTERNATIONAL PATTERNS[1]

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Substantive foci in the study of international relations have altered in time with changes in the international system and the coming of academic age of new generations of scholars.[2] Prior to World War I, the central substantive concepts were international law and diplomacy. Historiography was the major method, and, given the nature of the historical approach during this period, few theoretical generalizations emerged.

World War I revolutionized the study of international relations. The horrible consequences of this first modern war and the idealistic fervor of the war years were instrumental in overlaying the traditional concepts of international law and diplomacy with two new foci: current events and international organizations. Feeling that the citizen should be made aware of the international world—educated for world citizenship—and that he should be given the guidance that the diplomatic history and international law specialist did not provide, many international relations scholars began to accent contemporary affairs. This new interest, however, had no methodological underpinning except fidelity to the "facts," and involved few attempts to delineate recurring patterns of events. The "guidance" given to the student often turned out to be little more than special pleading.

The focus on international organizations also reflected an internationalistic viewpoint. International organizations were conceived of as the structural beginning of world government and as a mechanism for international understanding and peace. This focus articulated itself in descriptive studies of the structure and rules of international organizations, past and present, and blueprints for their alteration.[3]

Although there were indications of another conceptual shift beforehand,[4] World War II marks the domination of the study of international relations by two concepts: politics and power. This change is evident in the role given international organizations. During the interwar years, they were a central concept of international relations and much that happened was related to them. Now, international organizations are conceived of as only one and often a minor element of the milieu in which power politics and conflict take place.

With World War II, there was not only a sharp shift in conceptual foci but in conceptual scheme as well. The previous scheme involved, to borrow the Sprouts' distinction,[5] *concrete structures*. The concepts of central concern referred to readily describable institutions and officially recognized behavior with status in international law. Thus, terms such as nation-state, colonialism, the League of Nations, warfare, and treaties were woven into a framework for describing the international (or most often, European) system. The shift in conceptual scheme was from *concrete structures* to *analytic components*. The emphasis changed from describing past and present official organs and behavior to underlying forces, conditions, and patterned regularity in behavior. If any work can divide the old from this new trend, it is Quincy Wright's *A*

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[2] Kenneth W. Thompson, "The Study of International Politics: A Survey of Trends and Developments." *Review of Politics*, 14 (October 1952), 433–467. See also Dwight Waldo, *Political Science in the United States of America* (UNESCO, 1956).

[3] This is not to belittle a normative or policy approach to international relations. Although current work in empirical and quantitative international relations may appear more removed from moral commitments by its abstractness, the values of the researcher are involved no less than in traditional policy-oriented scholarship. If any difference between empirical and quantitative international relations on the one hand and that which has gone before on the other is to be drawn, it is not in a difference in moral commitments. Rather, the difference lies in making assumptions, logic, and data explicit so that others may test any conclusions drawn *regardless of their values*.

[4] See, for example, Frederick Schuman, *International Politics* (New York: McGraw-Hill, 1933), and H. D. Lasswell, *World Politics and Personal Insecurity* (New York: McGraw-Hill, 1935).

[5] Harold and Margaret Sprout, *Foundations of International Politics* (New York: D. Van Nostrand, 1962).

Study of War,[6] which accents politics, trends, themes, systematic methodologies, and the contribution of the social sciences to international relations.

The new conceptual scheme with its emphasis on analytic components is not divorced from the older scheme, as any current text on international relations would attest. Concepts central to the field before each of the World Wars still title the chapters of our books. Rather than conflicting, the two schemes are complementary. The heavy analytic emphasis of the new conceptual view of international relations has served to tie together the older concepts in a theoretical and explanatory nexus. This is the chief significance of Morgenthau's now classic *Politics Among Nations*.[7]

As it grew within the field, however, the new conceptual scheme has appeared increasingly out of focus. Being analytic, accenting forces, and highlighting patterns, the scheme begs a precision that it lacks. If power and nationalism are forces, how are they defined? How do we know when and with what magnitudes they are operating? If regularities in international behavior exist, what are they? How are such regularities determined? Such questions were crucial to the new scheme, but have not been answered by its developers.

The elaboration of an analytic conceptual scheme was an acknowledged move towards a science of international relations. In being theoretically explicit and talking in terms of causal relationships, the proponents of the post-World War II outlook were self consciously "scientific." But they were scientific largely in form and not substance. Knowledge could not be accumulated because there were few systematically organized findings. Generalizations could not be disproved because they lacked empirical specificity. And theories, such as the balance of power, could not be tested because they lacked logical or mathematical rigor.

Even as the analytic conceptual scheme was consolidating its hold on the study of international relations, two trends became evident within a decade after World War II. One trend involved a number of international relations scholars, who, persuaded of the salience of the social sciences and dissatisfied with the imprecision of "power" and "politics," began to fill out the newer analytic scheme with borrowed concepts of "conflict," "community," "perception," "communication," and "hostility." Symptomatic of this trend were the works of Deutsch, Wright, and Pool.[8]

A second trend comprised the invasion of the field by scientists and scholars trained in other disciplines and methodologies. Concerned about the consequences of another World War, dissatisfied with the dominant conceptual scheme, and unmindful or disrespectful of the graduate school varieties and prevailing methodology, social psychologists, economists, physicists, mathematicians, and sociologists began to write about the international system. Research by Klineberg, Cantril, Richardson, Rapapport, Guetzkow, Schelling, to name a few,[9] began to contest prevailing approaches and concepts.

If tied together by any thread, these trends involving the importation of social science concepts and the movement into the field of "outsiders" shared an emphasis on quantitative techniques and methodologies. Within and without the field, the elaboration and focusing of the prevailing analytic scheme has emphasized data, measurement, statistical arrays, and mathematical models. Attempts to add precision to the concepts and explicitness to the themes have been like a river winding from its source, with more or less breadth or depth from one study to the next, but generally tending in the same direction.[10]

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[6] Quincy Wright, *A Study of War*, 2 vols. (Chicago: University of Chicago Press, 1942).

[7] Hans J. Morgenthau, *Politics Among Nations* (New York: Knopf, 1948).

[8] Ithiel de Sola Pool, *Symbols of Internationalism* (Stanford: Stanford University Press, 1951); Karl W. Deutsch, *Nationalism and Social Communication* (New York: Wiley, 1953); Quincy Wright, *The Study of International Relations* (New York: Appleton-Century-Crofts, 1955).

[9] O. Klineberg, *Tensions Affecting International Understanding* (New York: Social Science Research Council, 1950); H. Cantril, *Tensions That Cause Wars* (Urbana: University of Illinois Press, 1951); L. F. Richardson *Statistics of Deadly Quarrels* and *Arms and Insecurity* (Pittsburgh: Boxwood Press, 1960); A. Rapoport, *Fights, Games, and Debates* (Ann Arbor: The University of Michigan Press, 1960); H. G. Guetzkow, "A Use of Simulation in the Study of Inter-Nation Relations," *Behavioral Science,* 4 (1959), 183–191; Thomas C. Schelling, *The Strategy of Conflict*. (Cambridge: Harvard University Press, 1960).

[10] The river simile can be used to make another point. Quantitative research in international relations does not presume an antithesis between traditional and quantitative scholarship. On the contrary the two are complementary. As the course of a river is determined by the fertile valleys through which it flows, so is quantitative research channeled by traditional insight and knowledge. Together the land and the river produce an abundant harvest and together the precision and re-

liability of quantitative scholarship combined with the rich comprehensiveness of traditional scholarship bear new fruit.

But conceptual sharpness through quantification has tilted the analytic scheme out of focus along another dimension. Concepts such as communication can be defined by mail flows, to be sure, but why not also or alternatively by telephone calls, telegrams, exports of periodicals and books, diplomatic notes, official conferences, or any summation or ratios formed from these activities? Indeed, once the door is opened to quantification, the quantifier finds himself in a dilemma similar to that of the verbalizer. Power and international politics can be and are differently defined according to the background and imagination of the scholar. With an infinity of variables and masses of data at his disposal, the quantifier can "operationalize" national power as the defense budget, men under arms, total energy resources, or national income, and politics as bloc membership, U. N. voting, military aid, or state visits as his predilection and interests dictate.

In spite of the eclecticism about variables that has infused quantification in international relations, there is an underlying rationality about operationalizing central concepts. For one thing, variables are selected as *indicators* of *underlying* processes or forces not directly measureable themselves. Thus, GNP per capita may be selected to measure economic development, mail to measure communications, and threats to measure conflict. Moreover, variables are selected to be as statistically uncorrelated as possible. Otherwise, the variables are redundant: one of them could do as well as any of the others. Finally, they are selected to have high validity. They should indeed be indicators of that they purport to measure.

Although few would deny the soundness of these requirements, they have led to a multitude of variables, even in the "operationalization" of the same concepts, for several reasons. One is that there has been no large scale mapping of the correlations and patterns of variation among potential indicators. When selecting indicators for an empirical study, quantifiers have had to use their judgement as to what potential measures were highly correlated. Are exports, exports per capita, or ratio of exports to GNP highly correlated indicators of transactions such that any one would be sufficient, or do they measure independent aspects of transactions between nations?

A second reason is that there have been no studies concerned with the validity of possible indicators. How do we know that GNP per capita is the best indicator of economic development? Perhaps we should agree with David McClelland[11] in selecting electricity consumption per capita as the central measure.

A final reason is that there have been few empirical attempts to simultaneously treat the whole conceptual framework. Concepts have been added to the analytic scheme, some have been operationalized and studied empirically, but always as discrete studies. The empirical interrelatedness of all these concepts, such as power, politics, conflict, transactions, communications, economic development, international organizations, perception, geography, and so on, have not been determined. It is not surprising that the choice of indicators for such concepts has been eclectic, since not only is it unclear as to how a variable relates to that which it is to measure, but the empirical patterns among the processes, structures, and forces being separated for study are themselves unknown.

The purpose of this paper is twofold. One goal is to contribute towards a refocusing of the analytic conceptual scheme employed in the study of international relations. Rather than being based on quantification alone, however, this refocusing will entail fastening major concepts to major patterns of variation in the empirical attributes and interactions of nations. The second and primary goal is to help and encourage a definition of the best indicators of these major concepts.

The remainder of the paper will propose several criteria for selecting a preferred set of indicators and a number of measures that more or less satisfy these criteria. The bulk of the discussion will consist of the evidence for selecting the indicators they measure.[12] This evidence is based on dozens of large scale analyses involving in total several hundred variables defining attributes and behavior of nations. The data cover the years ranging from 1836 to the 1960's. To give maximum visibility and comprehension to this evidence, the salient findings will be presented schematically in a number of tables. Those wishing more precise information about these findings will be referred to the proper source.

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[11] David C. McClelland, *The Achieving Society* (New York: D. Van Nostrand, 1961).

[12] At this point I will have to increasingly rely on a technical vocabulary to communicate the criteria and evidence. If the more traditionally trained scholar has gamely stayed with me so far, I beg his further indulgence. Technical terms I will use to a minimum, but I must employ such terms if any of the accumulated evidence is to be offered.

The primary technique involved in generating this evidence is factor analysis. The technique is appropriate for a large scale mapping of empirical concepts in a domain such as international relations and for delineating the best indicators of those concepts.[13]

I. CRITERIA FOR A SET OF BASIC INDICATORS

What criteria might a set of variables satisfy to be preferred or basic indicators of major attribute and behavioral concepts of nations? Let me offer seven.

1) *Data must be readily available on the indicators for most nations.* An indicator such as the "number of secret agents" would hardly be a feasible choice.

2) *Data should be comparable on a basic indicator.* For some variables, the definitions under which data are aggregated vary from nation to nation. Gross national product and national income, for example, are measures for which the data do not mean the same between, say, Poland and France, and for which exchange rate conversion to common currency adds additional elements of noncomparability.

3) *Each basic indicator should be measured on a ratio or interval scale.* Each nation will then usually have a distinct value on the indicator and the indicator will be usable in all mathematical models, including those employing calculus. If the choice of indicator must be between ordinally scaled variables, then that variable having more disrete values (more ranks) is to be preferred.

4) *Each basic indicator should index a pattern of variation among nations.* A pattern of variation is a cluster of intercorrelated variables and a basic indicator should be central to the pattern.[14] Why this should be so may not be immediately evident. Consider "electricity generated," which is a variable related to *economic development* and *size* patterns.[15] How are we to interpret this variable's correlation with other variables such as trade, threats, tourists, or domestic unrest? The correlation may be due to variance in economic development, in size, or in both. However, were we to use a variable indexing only *one* pattern of nations, then the variance involved in that variable's correlations would not be ambiguous. Moreover, since the variable indexes a pattern of intercorrelated variables, *what correlations or findings hold for the variable hold also for the other variables involved in the pattern.* This enables comparison of results for variables in the same pattern and facilitates convergence on scientific laws and theories.

5) *The set of basic indicators should encompass the major variance among nations.* That each indicator should index a pattern implies that the major patterns reflecting most of the variation among nations will be indexed by the set as a whole. With knowledge of such a set of basic indicators, the student of international relations can be sure of covering the major differences and similarities among nations. These basic indicators would be useful, for example, in relating national systems to foreign conflict, national societies to foreign policy, or international behavior to nation attributes. Most importantly, and apropos the introduction to this paper, the set should serve as a collection of indicators with known correlates and from which specific variables can be selected to operationalize the concepts of a theory or hypothesis.

6) *The set of basic indicators should be divided into two types: basic indicators of the attributes of nations and basic indicators of the relations between nations.* This distinction is clearly the same as that dividing comparative politics from international relations—concern about the characteristics of nations versus concern about their relations with each other. For our purposes, the need for two types of basic measures rests on an analytical difference. Nations are our units of analysis. Basic indicators of their attributes then define the variation among these units. Our major concern is not with the units themselves (as it is with comparative politics or comparative economics, for example), but with the behavior of the units vis-a-vis each other. For this primary concern we require a different set of basic indicators: those of nation behavioral patterns. Perhaps, the analytical difference between basic nation *attribute* indicators and basic nation *behavior* indicators can be seen best in terms of two matrices. The nation attribute matrix is a nation by attribute table, where each cell refers to the value of a nation on an attribute, such as GNP, trade, or migrants. These tables are standard in presenting data on nations, such as in the United Nations *Statistical Yearbook*.

The nation behavior matrix is a *dyadic* nation by behavior table. Each cell of the table refers to the action of a specific nation, such as the U. S., towards a specific other nation, such as the USSR, for a behavior defined by the column of the table. Let the letters A, B, C, etc., stand for different nations. Then such a dyadic table would look like the following:

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[13] Elsewhere I have published a paper on "Understanding Factor Analysis" *Journal of Conflict Resolution,* 11 (December, 1967), 444–480, which might be helpful auxiliary reading for the remainder of this paper.

[14] In some other papers I have used the term "dimension" rather than "pattern." The latter term is preferred here because it may communicate better the basic idea than does the more mathematical and abstract notion of dimension.

[15] That economic development and size are patterns of nations will be established subsequently.

	EXPORTS	THREATS	EMIGRANTS	TOURISTS	AID	STATE VISITS	MAIL
A→B							
B→C							
A→C							
D→A							
E→F							
•	•	•	•	•	•	•	•
•	•	•	•	•	•	•	•
•	•	•	•	•	•	•	•

The arrow in A→B means A is the actor and B the object. For the exports variable for example, the data on A→B would be A's exports to B.

7) *That each basic indicator indexes a pattern must be established empirically, systematically, and reliably.*

At this stage in the growth of a quantitative international relations, it would be foolhardy to suggest anything more than a first provisional list of indicators, more or less satisfying these seven criteria. Further studies and the application of these indicators will refine and alter this list. With this tentativeness in mind, I would like to suggest the following major patterns (empirical concepts) of nations and their indicators.

A. *Attribute Indicators*	*Major Patterns*
1. energy consumption/population	economic development
2. population	size (power bases)
3. bloc membership[16]	political system
4. killed in foreign violence	foreign conflict behavior
5. killed in domestic violence	domestic conflict behavior
6. Roman Catholics/population	Catholic culture
7. population/area	density

B. *Behavior Indicators*	*Major Patterns*
1. tourists A → B	salience
2. emigrants A → B/A's population	emigration and communications
3. weighted UN voting distance A↔B	UN voting
4. students A → B/A's students to all nations	foreign student
5. exports A → B/A's GNP	export
6. IGO A↔B/A's IGO membership	international organizations
7. military violence (factor) score A → B	official conflict behavior
8. embassy or legation A → B/A's total embassies or legations	diplomatic representatiion
9. U.N. voting distance on "Self Determination" issue pattern A↔B	self-determination voting
10. anti-foreign behavior (factor) score A → B	anti-foreign demonstrations

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[16] Bloc membership refers to western, neutral, or eastern bloc membership.

The next section will present evidence for selecting the set of attribute indicators and the subsequent section will do the same for the behavior indicators. A final section will discuss some of the implications of the combined set of indicators.

II. ATTRIBUTE MEASURES

As part of the research of the Dimensionality of Nations Project (DON), and as discussed elsewhere,[17] a factor analysis of 236 measures of eighty-two nations for 1955 data delineated fifteen distinct patterns of variation. These fifteen patterns account for seventy-seven percent of the differences and similarities of nations on the data. Many of these patterns are trivial, however, and can be ignored in defining the primary empirical concepts and their basic indicators. The *major* patterns[18] consist of the seven shown in Figure 1, which account for about fifty-six percent of the *total* variance of nations on the 236 variables, and seventy-two percent of the total variance tapped by the original fifteen patterns.

In the analysis variables were organized by *type* of attribute. For example, the agricultural type involved agricultural workers/economically active population, agricultural production/GNP, and agricultural population/population variables. To keep the size of the Figure manageable, the relationships between variables and patterns are shown in terms of attribute types.

The names given to the seven patterns are based on those variables most highly involved in each. They are meant to be descriptive and mnemonic labels only and are not to be interpreted as presumed causes underlying the patterns.

The results given in Figure 1 define the *uncorrelated* patterns of interrelated variables. The patterns shown, however, will depart from reality as the empirical patterns in the data are themselves correlated. Rather than risk presenting artifactual results due to an a priori requirement imposed on the patterns found, an additional analysis relaxing this restriction was done.[19] The best definition of the correlated or uncorrelated data patterns is shown in Figure 2.

This secondary analysis brought forth the same seven patterns, although the relationship of the variables to the patterns was slightly altered. These correlated patterns best define the central variables and are shown in Figure 2, along with the highest "loading" variables for each pattern. A "loading" is a coefficient measuring the amount of variation of a variable involved in a pattern. The higher the loading, the higher the involvement. Negative loadings mean inverse relationship between variable and pattern.[20] The sum of squared loadings shown in the figure and which is calculated for all 236 variables measures the relative size of the patterns. The higher this sum, the more the variation in the variables involved in the pattern.

Those variables that most satisfy the seven criteria for a basic indicator are shown blocked in Figure 2.

How reliable are these seven patterns? Several similar analyses of nation attributes have been done by others, among which are those of Cattell,[21] Berry,[22] and Russett.[23] Table 1 gives

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[17] Jack Sawyer, "Dimensions of Nations: Size, Wealth, and Politics," *American Journal of Sociology,* 73 (September 1967), 145–172; R. J. Rummel, "The Dimensionality of Nations Project," in Richard Merritt and Stein Rokkan (eds.), *Comparing Nations* (New Haven: Yale University Press, 1966), pp. 109–129; R. J. Rummel, *Dimensions of Nations,* forthcoming, 1969.

[18] These are orthogonally (varimax) rotated factors. The initial solution was determined using the principal axes technique with unities in the diagonal of the correlation matrix. Correlations were product moment and were calculated on variables with an average of about seventeen percent missing data. All factors with positive eigenvalues were extracted and rotated and all positive eigenvalues exceeded unity. Further details and results are given in Rummel, *Dimensions of Nations.*

[19] This analysis comprised the oblique (biquartimin) rotation of the initial fifteen factor solution. Because of the number of variables involved, the rotation was carried out in stages of ten major cycles each until there was no major alteration in loadings from one stage to the next.

[20] Because of small correlations between the patterns, the loadings can be loosely interpreted in our oblique rotation as the correlation between variable and pattern.

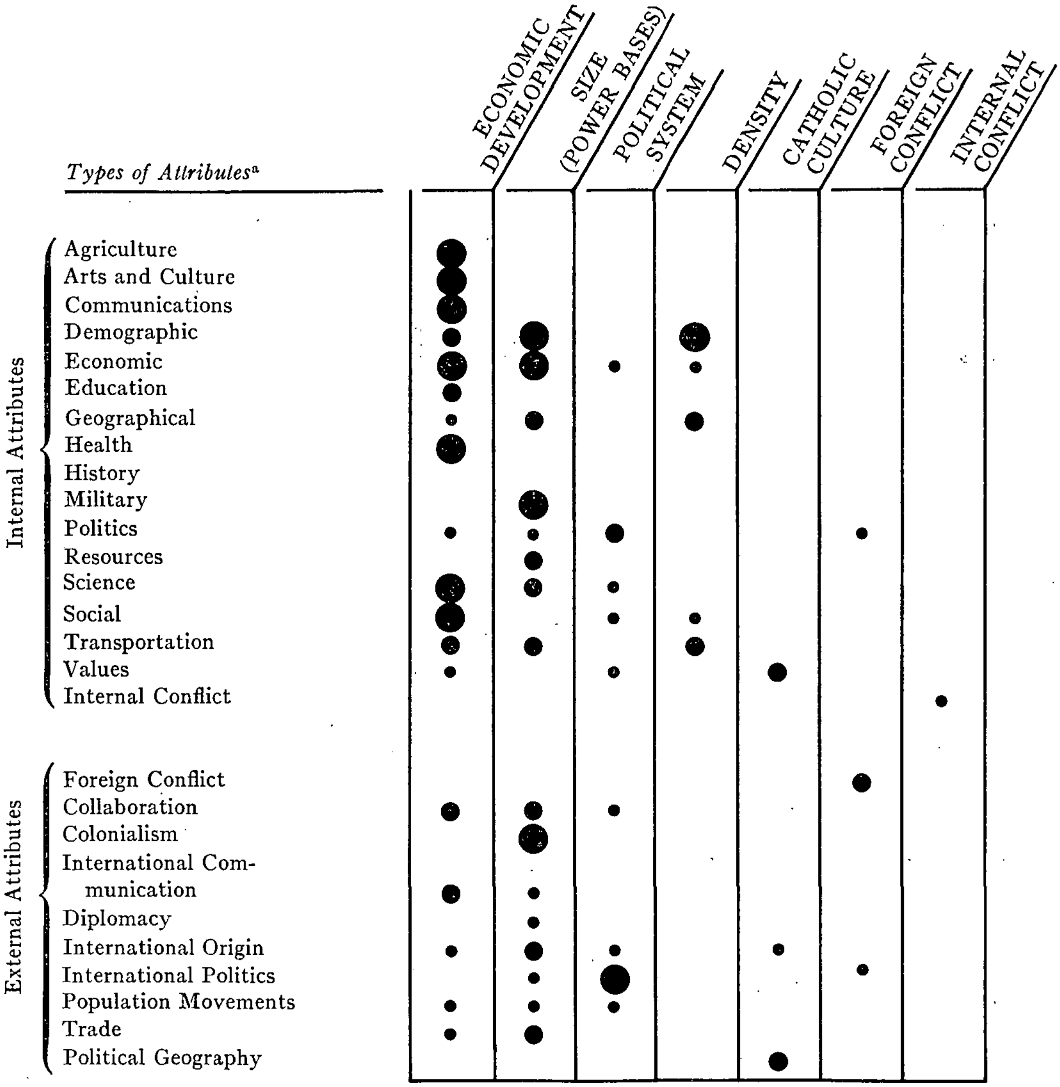
[21] Raymond B. Cattell, "The Dimensions of Culture Patterns of Factorization of National Characters," *Journal of Abnormal and Social Psychology,* 44 (1949), 443–469; Raymond B. Cattell, H. P. Hartman, and H. Breul, "An Attempt at More Refined Definition of the Cultural Dimension of Syntality in Modern Nations," *American Sociological Review,* 17 (1952), 408–421; Raymond B. Cattell and Richard Gorsuch, "The Definition and Measurement of National Morale and Mortality," *The Journal of Social Psychology,* 67 (1965), 77–96.

[22] Brian J. L. Berry, "An Inductive Approach to the Regionalization of Economic Development," in Norton Ginsburg (ed.), *Essays on Geography and Economic Development* (Chicago: University of Chicago Press, 1960).

[23] Bruce M. Russett, *International Regions and*

FIG. 1.

Patterns[b] of Nation Attributes



● ≥75%
● ≥50%
• ≥25%
variation in at least one of the variables categorized under the type of attribute is accounted for by the pattern.

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[a] These types categorize 230 variables. A category not shown included six measures of data error.

[b] Orthogonally rotated factors.

Fig. 2.

Correlated Patterns[d] of Nation Attributes

DON Patterns	Sum of Squared Loadings		Highest Loaded Indicators[c]
ECON. DEV.	63.4	1.05	Telephones per cap.
		1.02	Agricultural popula./population
		1.02[a]	Energy consump./popula.
SIZE (POWER)	36.9	1.03	Population × Energy prod.
		1.02	National income
		1.01	Gross national product
		1.00	Population
POLITICS	23.5	1.16	English titles transl./foreign titles transl.
		1.12	English titles transl./Russian and English titles transl.
		.90	Bloc membership
FOR. CONF.	12.2	.89	Threats
		.88	Accusations
		.78	Killed in foreign violence
DOM. CONF.	11.1	.80	Killed in domestic violence
		.75	General strikes
		.74	Riots
CATH. CULT.	10.9	.86	Roman Catholics/popula.
		−.83	Air distance from U.S.
		.79	Medicine NGO/NGO memberships
DENSITY	10.4	.95	Population/area
		.76	Arable land/area
		.67[b]	Road length/area

74.7% of total sum of squared loadings for 15 patterns.

[a] Literacy also has a loading of 1.02.
[b] R. R. length/area, also has a loading of .67.
[c] Suggested basic indicators shown in blocks.
[d] Primary pattern matrix.

the (intraclass)[24] correlation between the DON results and those of each of the studies cited.[25] In all cases of comparison in Table 1, we find the correlation quite high, suggesting that the DON patterns as a whole have also been delineated in other empirical studies.

International Integration (Chicago: Rand McNally, 1967).

[24] The intraclass correlation coefficient measures both profile and magnitude correlation, while the product moment measures only profile correlation. The intraclass is therefore a more stringent measure of reliability than the often employed product moment. See Section 12.3.3 of my *Applied Factor Analysis* (Evanston: Northwestern University Press, 1969) forthcoming.

[25] The technique of comparison employed was Ahmavaara's transformation analysis. See Yrjo Ahmavaara, *On the Unified Factor Theory of Mind* (Helsinki: Annales Akademiae Scientiarum Fennicae, 1957). The DON fifteen factors were first rotated to a least squares fit with those of another study. Then, the intraclass correlation was calculated between all the DON loadings and those of the other study.

TABLE 1. INTRACLASS CORRELATION BETWEEN DON CORRELATED PATTERNS AND OTHER STUDIES

Study			DON 15 Patterns
Russett:	8	patterns	.93
	1960c	data	
	54	variables	
	82	nations	
Berry:	5	patterns	.96
	1955c	data	
	43	variables	
	95	nations	
Cattel and Gorsuch (1965):	14	patterns	.90
	1960c	data	
	51	variables	
	52	nations	
Cattell, *et al.* (1952):	12	patterns	.77
	1837–1937	data	
	72	variables	
	40	nations	
Cattell (1949):	12	patterns	.78
	1837-1937	data	
	72	variables	
	69	nations	

Are the individual patterns reliable? Figure 3 shows the (product moment)[26] correlation between the seven DON dimensions and those of the other studies. Patterns of the other studies having correlations less than .50 (less than twenty-five percent overlap) with the DON seven are not shown. How naming patterns is an imperfect art can be seen from the different labels applied in the various studies.[27]

The number of studies in which each pattern appears to the number in which it could appear is[28]

economic development = 5/5,
size = 5/5,
politics = 3/4,

As can be seen from Figure 3, the DON patterns are quite similar to those of Russett. In order to extend our view of the reliability of the DON patterns, we might note some additional comparisons that Russett has done[29] with studies of Robinson,[30] Berry,[31] and Gregg and Banks.[32] These are shown in Figure 4. In each case, an image of Russett's patterns is found in the other studies. Since the (intraclass) correlation between DON's results and Russett's is .93, those patterns of other studies similar to Russett's should be also similar to those of DON.

In sum, the comparisons in Table 1 and Figures 3 and 4 suggest that the DON patterns are reliable individually and in total and that variables selected to index these patterns *are indicators of relationships generally found in cross-national data.*

What about data error or bias distorting the empirical patterns? Random error should cause us little concern. Its effect would be to decrease the loadings without obscuring the outlines of the patterns. Moreover, the comparisons with the results of other studies should buttress our confidence in the patterns independence of random error. Systematic error—biased data—is another problem, however, since all the data used in the various studies are from much the same sources, error might bias *all* the studies in the same direction.

Several measures of systematic error were developed[33] and included in the 236 variable factor analysis. The correlation of these error measures with the patterns would then indicate which possible sources of error were related to what patterns. Figure 5 shows the results. The "censorship" and "embassies and legations" error measures were included to index possible bias in the conflict data. Embassies and legations in a nation measures the world interest in that nation and the likelihood of conflict news about that nation being published. As can be seen from Figure 5, the *economic development* and *politics* patterns are the only ones involving measures of systematic error.

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[26] Correlations were computed after the DON fifteen factors were rotated to a least squares fit to those of the other study.

[27] I have relabelled, for clarity, Cattell and Gorsuch's *op. cit.* (1965), factors 3 and 14.

[28] If the variables involved in a pattern were not included in the analysis of the other study, then the pattern cannot be compared.

[29] Russett, *op. cit.* I wish to express my appreciation to Russett for communicating these comparisons to me before publication.

[30] John Robinson, "Multidimensional Analysis as a Comparative Framework for Poltical Systems," (Ann Arbor: University of Michigan, 1965, unpublished manuscript).

[31] Berry, *op. cit.*

[32] Phillip M. Gregg and Arthur S. Banks, "Dimensions of Political Systems: Factor Analysis of *A Cross-Polity Survey*," this REVIEW, 59 (1965), 602–614.

[33] R. J. Rummel, "Dimensions of Error in Cross-National Data," in Raoul Naroll and Ronald Cohen (eds.), *Handbook of Method in Cultural Anthropology*, forthcoming, 1969.

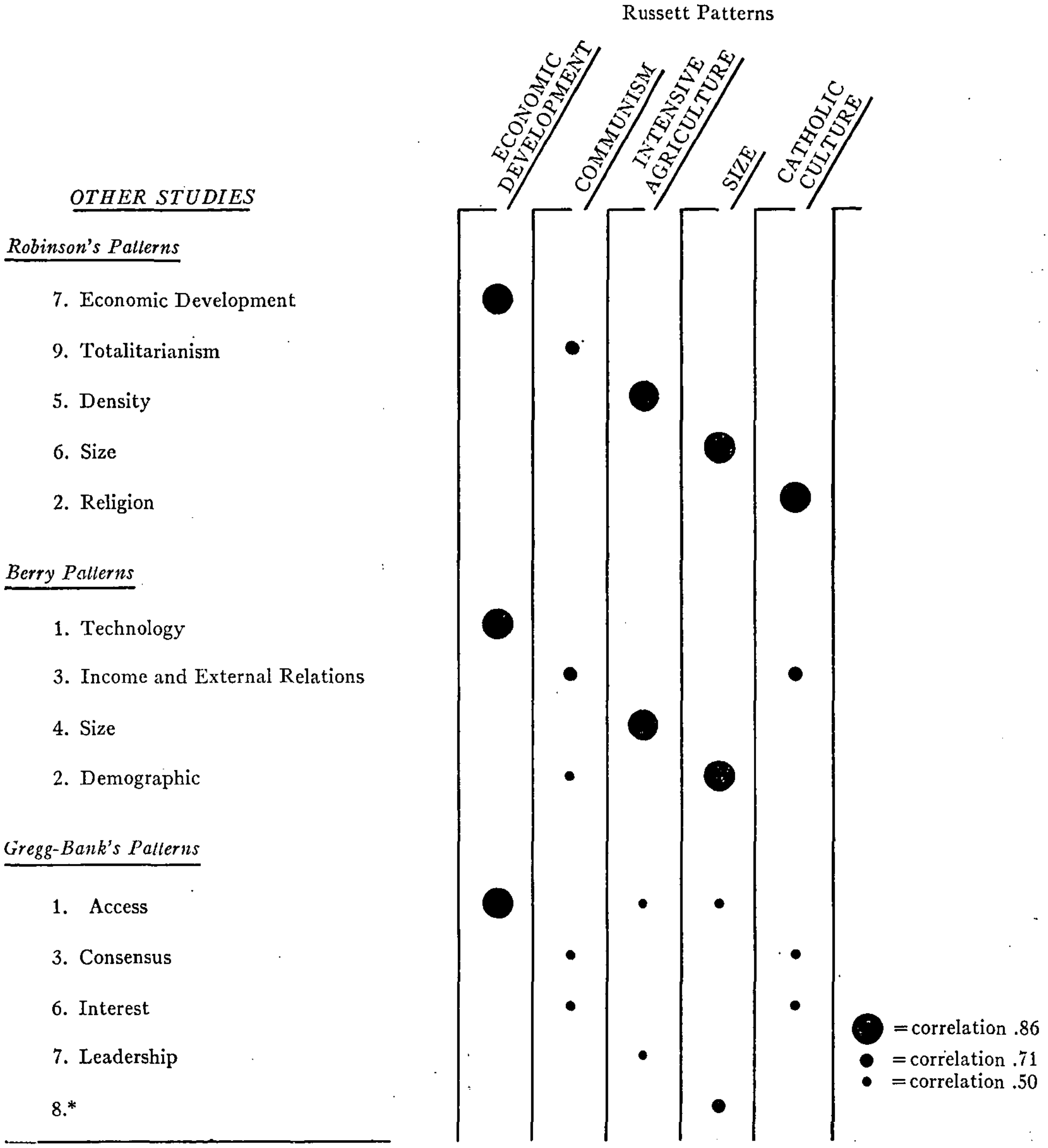
FIG. 3. Comparison of DON Correlated Patterns with those of Other Studies.

OTHER STUDIES	ECONOMIC DEVELOPMENT	SIZE (POWER BASES)	POLITICS	CATHOLIC CULTURE	DENSITY	FOREIGN CONFLICT	DOMESTIC CONFLICT
Russett Patterns							
1. Economic Development	⬤					|	|
4. Size		⬤				|	|
2. Communism			•			|	|
5. Catholic Culture				●		|	|
6. Intensive Agriculture					⬤	|	|
Berry Patterns							
1. Technology	•		|			|	|
3. Income and External Relations		•	|	•		|	•
2. Demographic	•		|	•		|	|
4. Size			|		●	|	|
Cattell (1965) Patterns							
1. Vigorous Development	•		•				
5. Size		•					
14. Asian Culture							•
2. Complex System				•			
3. Catholic Culture				•			
Cattell et al (1952) Patterns							
2. Vigorous Development	•						
6. Ferment		•			•		
5. Rationalism				•		•	
3. Cultural Pressure						•	
11. Morality							•
Cattell (1949) Patterns							
7. Vigorous Development	•						
12. Morality		•					
1. Size			•				•
3. Affluence					●		
2. Cultural Pressure						●	

⬤ = correlation ≥ .86
● = correlation ≥ .71
• = correlation ≥ .50
| = variables defining the column pattern were not included in the study

Catholic culture = 4/5,
density = 4/5,
foreign conflict = 2/3,
domestic conflict = 3/3.

FIG. 4. Comparison of Russett's Patterns with those of Other Studies.

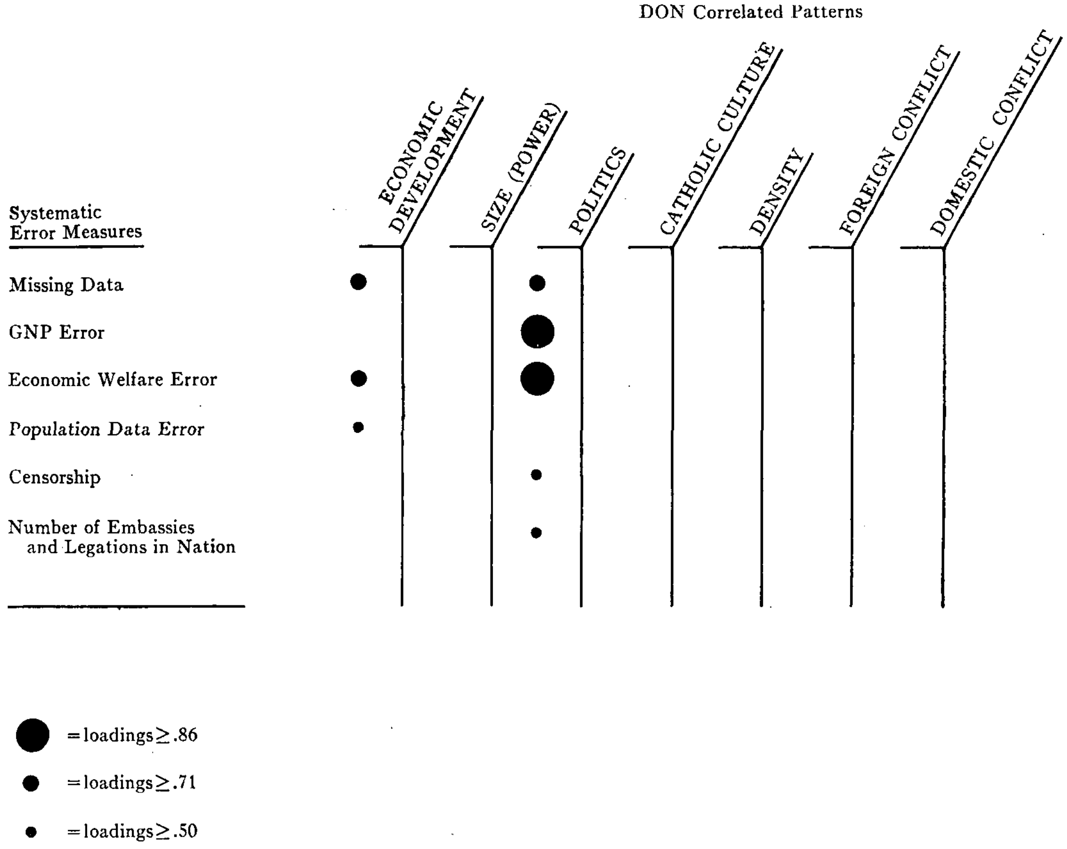


* A small factor unreported by Gregg and Banks in their published results.

A close look at the error measures related to the *economic development pattern,* however, would indicate that the attributes of nations reflected in this pattern are more a cause of this error than the other way around. For one thing, the variance in the variables involved in the pattern, such as the proportion of the population in agriculture, GNP per capita, telephones per capita, vehicles per capita, literacy, newspaper circulation per capita, and the ratio of proteins to calories, far exceeds the variance in possible systematic error. Nations should have nearly the same relative ranking on such measures with or without the error variance. Moreover, it makes good theoretical sense to argue that the amount of missing data and poor quality of economic and population data reflects low economic development—inadequate statistical training and investment in statistical services—rather than the concept of economic development reflecting poor data.

The relationships between the *politics* dimension and the error measures can be rationalized in the same way. Many of the variables defining

FIG. 5. Loadings of Systematic Error Measures.



politics have variance exceeding that of the error. Moreover, for some variables the data source is known to be unaffected by such possible error, as with bloc membership (dependent on military treaties), freedom of group opposition, or voting system measures. Moreover, it also is more plausible to argue that the politics pattern is the source of biasing error, rather than the other way around. Nonetheless, the error measure loadings of Figure 5 in conjunction with the general lack of high relaibility for the politics pattern in the comparisons of Figure 3 and 4 suggest that a politics pattern exists, but that its nature is not well delineated.

One additional finding can be related to the question of error. In Figure 5, the missing data measure is correlated with economic development and politics patterns. Will these patterns emerge, therefore, if we exclude the nations and variables with most missing data? The answer is yes. By eliminating variables and nations with missing data, the average of seventeen percent missing data per variable was reduced to an average of three percent for seventy-one nations and 153 variables. This latter data was then analyzed as was that for 236 variables. The comparison of the results is shown in Figure 6.[34] The (intraclass) correlation between both sets of results is .96 and each of the seven patterns is well defined.

In his factor analysis, Cattell[35] was also plagued with missing data. To check the invariance of the patterns he found, he eliminated nineteen nations and reanalyzed the resulting data on forty nations and seventy-two variables.[36] As in our case, the patterns came out largely the same. Missing data, then, do not have significant influence on our delineation of attribute patterns.

Finally, how correlated are the suggested attribute basic indicators? We certainly do not want highly correlated indicators, otherwise they can hardly reflect distinct patterns. The

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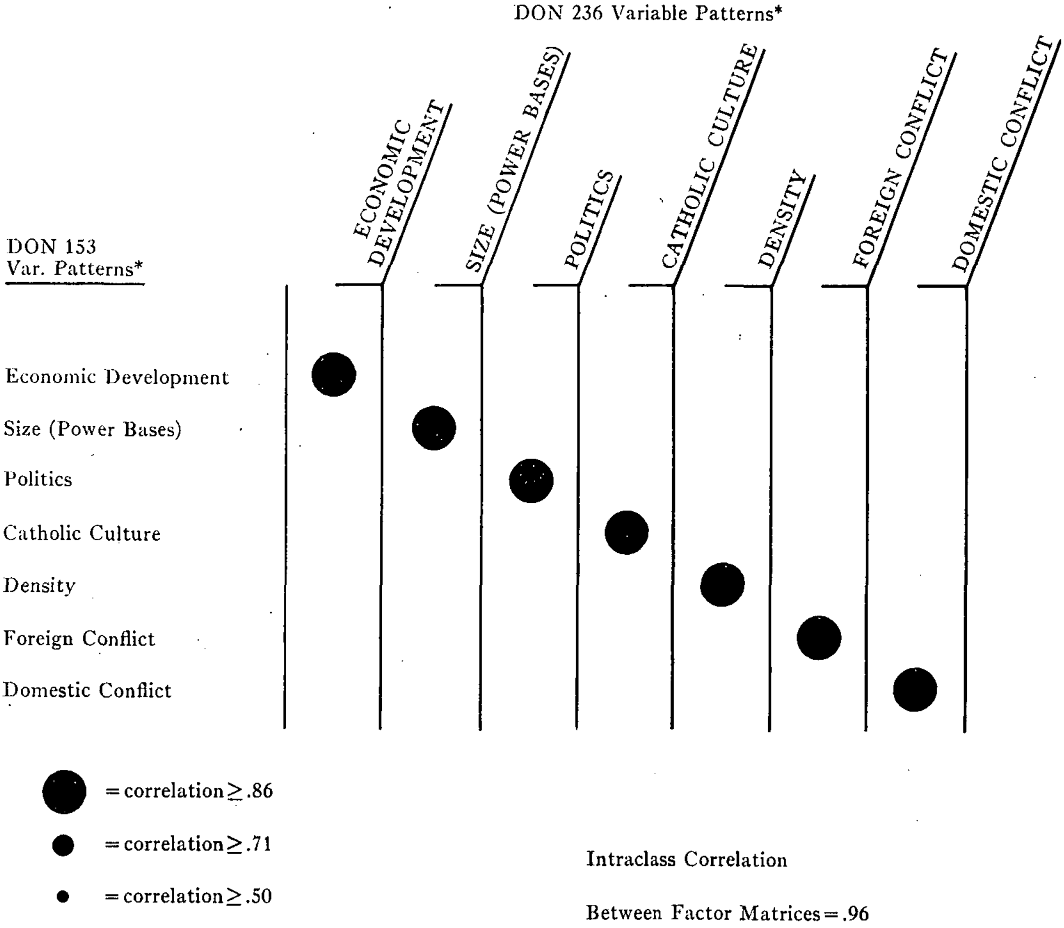
[34] The technique of comparison was the same as in the previous comparisons.

[35] Cattell, *op. cit.* (1949).

[36] Cattell, Hartman, and Breul, *op. cit.*

FIG. 6. Comparison of 236 and 153 Variable Patterns.


DON 236 Variable Patterns*
ECONOMIC DEVELOPMENT
SIZE (POWER BASES)
POLITICS
CATHOLIC CULTURE
DENSITY
FOREIGN CONFLICT
DOMESTIC CONFLICT
DON 153 Var. Patterns*
Economic Development
Size (Power Bases)
Politics
Catholic Culture
Density
Foreign Conflict
Domestic Conflict
● = correlation ≥ .86
● = correlation ≥ .71
● = correlation ≥ .50
Intraclass Correlation
Between Factor Matrices = .96

* Oblique primary pattern matrices

ideal is a set of basic indicators, each virtually uncorrelated with the other. Table 2 presents the coefficient of determination between all basic indicators. The coefficient measures the *proportion* of variation they have in common. The highest coefficient is .12 and most coefficients are virtually zero.[37] *Each basic indicator thus measures a nearly statistically independent component of variance in nation attributes.*

TABLE 2. COEFFICIENT OF DETERMINATION* BETWEEN BASIC INDICATORS

Basic Indicators	1	2	3	4	5	6
1. Energy consump./population		78	78	74	74	73
2. Population	.03		82	77	77	77
3. Bloc membership	.00	.00		77	77	77
4. Killed in foreign violence	.00	.05	.04		77	72
5. Killed in domestic violence	.00	.09	.03	.05		72
6. Roman Catholics/population	.06	.02	.12	.03	.03	
7. Population/area	.01	.10	.02	.02	.00	.00

* This is the product moment squared and gives the proportion of variance in common between two basic measures. The upper triangular matrix gives the sample size for each coefficient. Data are for 1955.

III. BEHAVIORAL MEASURES

In order to define patterns and indicators of international behavior, an analysis different from that above is required. Rather than analyzing

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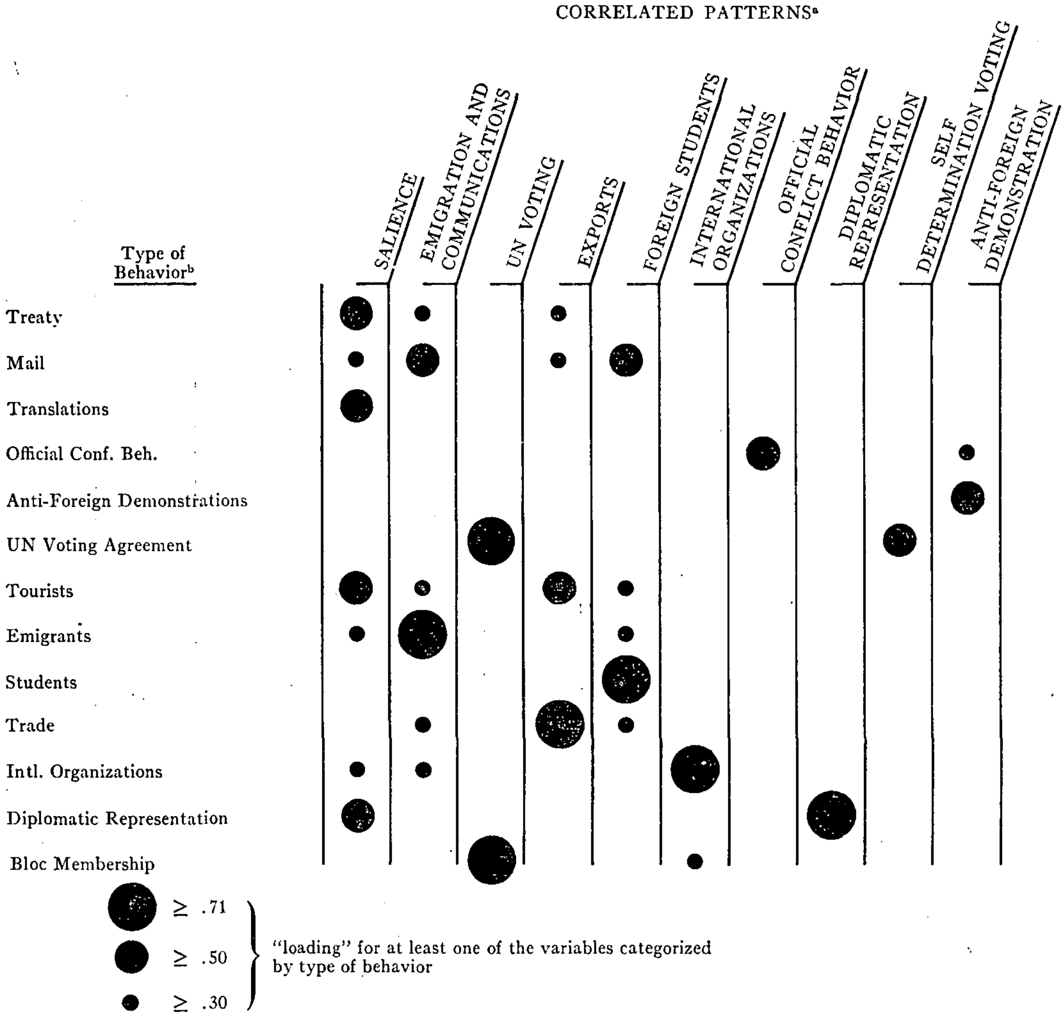
[37] For the size pattern, a basic indicator might be population times energy consumption. It has a higher loading than population, but it also has a coefficient of determination of .37 with energy consumption per capita. For this reason population is a preferred basic indicator.

Fig. 7. Patterns of Nation Behavior.



[a] Oblique primary pattern matrix

[b] These types categorize forty measures

data for nations on *attributes*, data for dyads (nation A→B, B→C, etc.) on *behavior* variables are analyzed.

Figure 7 displays the results of analyzing forty behavior variables for 346 nation dyads on 1955 data. These results are a composite of four parallel analyses:

1) Raw data on a random sample of 164 dyads.
2) Transformed data[38] on the above sample.
3) Raw data on a selected sample of 182 dyads. Fourteen nations were selected to represent high, middle, and low scores on the attribute patterns of nations (Figure 1) and major cultural and regional groupings.[39] Then taking all possible dyads (A → B) for these nations produced the selected sample.
4) Transformed data on the above sample.

Each of the four sets of data were similarly

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[38] Data were transformed to increase the normality of the distributions and to remove extreme values. This reduces the effect on the results of a few high behavior dyads, such as UK→USA and USA→UK.

[39] Russett, *op. cit.* Raymond B. Cattell, "The Principal Culture Patterns Discoverable in the Syntal Dimensions of Existing Nations," *The Journal of Social Psychology*, 32 (1950), 215–253.

FIG. 8.

Dyadic Analyses Research Design

		Sample	
		Random	Selected
Data	Raw	164 nation dyads	182 nation dyads
	Transformed	164 nation dyads	182 nation dyads

Intraclass Correlation

Between Results

	Raw Selected Sample
Raw Random Sample	.84
Transformed Random Sample	.87
Transformed Selected Sample	.78

analyzed.[40] The *raw selected* sample results were then selected as a base[41] to which were compared each of the patterns from the other samples.[42] Figure 8 displays the four samples, and the (intraclass) correlation between the raw selected correlated *patterns* and the others.

With this four-sample design specified, I can now give meaning to the composite "loadings" in Figure 7. The three sets of patterns found for the transformed selected, transformed random, and raw random samples were fitted to the raw selected sample patterns.[43] Then the "loadings" given in Figure 7 were computed as the geometric mean of the loadings of a variable on the corresponding patterns for all four samples. The "loadings" of Figure 7 therefore represent the most stable findings across the four samples. These composite "loadings" and (intraclass) correlation comparisons of Figure 8 suggest that we can rely on the raw selected sample for selecting a set of behavioral indicators, but with an eye towards the loadings of a variable in the other three samples.

Table 3 gives the correlated patterns of the raw selected sample. The suggested basic indicators have high loadings for all four sample results and the geometric means of these loadings are shown in Table 4.

Four of the selected basic indicators need some clarification. The weighted U.N. voting distance A↔B indicator is the Euclidean

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[40] The same factor analysis and rotation procedures were used as for the attribute analysis. See footnotes 18 and 19.

[41] This sample has the most interesting dyads and raw data patterns. Moreover, indicators selected from the analysis of this sample will be simpler to interpret and employ.

[42] The same factor comparison method was used as employed for the comparisons of Figure 3.

[43] The patterns of each of the three samples were rotated to a least squares fit to those of the raw selected sample. This maximized the linear correspondence of the results and washed out irrelevant effects on the oblique rotations which were done independently for each sample.

TABLE 3. CORRELATED PATTERNS OF NATION BEHAVIOR

DON DYADIC PATTERNS (Raw Selected Sample)	Sum of Squared Loadings		Highest Loaded Measures[a]
Salience	5.3	.87	Translation A→B / A's total translations
		.84	Translations A→B
		.79	Tourists A→B
Emigration and Communication	4.9	.98	Emigrants A→B / A's total emigrants
		.84	Emigrants A→B / A's population
		.77	Mail A→B
U.N. Voting	4.2	.92	Weighted U.N. voting distance A↔B
		−.81	Common Bloc membership
		−.79	Common Bloc leadership
Foreign Students	3.8	.95	Students A→B / A's students to all
		.89	Students A→B
		.82	Mail A→B / A's mail to all
Exports	3.7	.93	Exports A→B / A's GNP
		.89	Tourists A→B / A's population
		.83	Exports A→B / A's exports
International Organization	2.9	.86	IGO A↔B / A's IGO[b] membership
		.79	NGO A↔B / A's NGO[c] membership
		.68	IGO A↔B
Official Conflict Behavior	1.9	.87	Military Violence factor score A→B
		.84	Negative communication factor score A→B
Diplomatic Representation	2.0	.95	Embassy or Legation A→B / A's total Embassies or Legations
		.84	Embassy or Legation A→B
Self Determination Voting	1.7	.88	U.N. voting distance on "Self Determination" pattern A↔B
		.70	U.N. voting distance A→B
Anti-Foreign Demonstration	1.3	−.71	Negative sanctions factor score A→B
		.65	Anti-foreign behavior factor score A→B

[a] Suggested basic measures shown in blocks.

[b] IGO: Intergovernmental international organizations.

[c] NGO: Nonintergovernmental international organizations.

distance[44] between nations on the uncorrelated pattern of U.N. voting in the Xth (1955) Sessions of the General Assembly. To determine these distances, Assembly committee and Plenary sessions first were separately analyzed to define the issue patterns of roll call voting. Then, those issues central to the committee and Plenary patterns were combined and reanalyzed. The resulting patterns describe the most general clustering of issues in the U.N. and were used to calculate the weighted U.N. voting distances between nations.[45] "Weighted" means that in

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[44] The Euclidean distance, d_{AB}, between two nations, A and B, in the space of p orthogonal factors (uncorrelated patterns) $S_1, S_2, \ldots, S_l, \ldots, S_p$ is

$$d_{AB} = \left[\sum_{l=1}^{p} (S_{Bl} - S_{Al})^2\right]^{1/2}$$

where S_{Al} and S_{Bl} are the factor scores of nations A and B on the lth factor.

[45] The issue patterns were internationalism, admission of new members, self determination, se-

computing the distances each pattern was weighted by the variance in roll call votes for which it accounted.

The U.N. voting distance on a "self determination" pattern measure was derived similarly to the above. "Self determination" was one of the patterns of U.N. voting to emerge from the patterns of U.N. voting to emerge from the U.N. analysis. It involved issues concerning the competence of the U.N. to decide whether a Non-Self-Governing Territory has attained a full measure of self-government (A/3087), a proposed article to the International Covenant on Human Rights on the right of nations to self-determination (A/C.3/L.478/point 2), a proposal to set up a working committee to consider the previous issue (A/C.3/L.477/Rev. 1), and a proposal that would in effect postpone a decision on the previous issue (A/C.3/L.479/Rev. 1). Euclidean distances between nations on this pattern then made up the variable used in the dyadic behavior analysis.

The third and fourth indicators needing clarification are the military violence (factor) scores and anti-foreign demonstration (factor) scores. A previous analysis[46] of all dyads (340) having foreign conflict on any one of sixteen variables[47] determined four raw data patterns of conflict behavior: military violence, negative sanctions, negative communications, and anti-foreign behavior. Scores for dyads on these patterns were included in the dyadic behavior analysis. As noted, the military violence and anti-foreign demonstration scores turned out to be basic indicators. The best single indicator of the military violence scores is the number of discrete military actions or clashes A → B. For the anti-foreign behavior scores, the number of anti-foreign demonstrations is a good indicator.

Returning to the ten behavioral patterns of Figure 7 and Table 3, we might ask: How reliable are the behavioral patterns? Unfortunately, no previous analyses of dyads on such behavioral variables have been published. The four-sample analyses reported here give some idea of the stability of the patterns for different samples and data, but we need a number of independent dyadic analyses by different researchers before we can have the degree of confidence in the behavioral patterns that we can have in the seven attribute patterns of the previous section.

What about data error or bias distorting the behavioral patterns? Similar to the systematic error analysis of the seven attribute patterns (Figure 5), six measures of possible sources of bias were included in the raw selected sample analysis. The error measures are

1) Exports A → B minus imports B ← A. This measures the reliability of trade data, and possibly of other behavioral data as well.

2) Exports A → B imports B ← A/trade average (A → B) + (B ← A). This is a standardization of the above error measure by the amount of trade.

3) Embassies and legations in A and B. This is a measure of the world interest in a pair of nations, and thus the possibility that data, such as that on conflict, might be omitted from the sources.

4) Energy Consumption per capita for A and B. Energy Consumption per capita is the suggested basic indicator of economic development and we have found (Figure 5) that economic development is correlated with some possible sources of systematic error. Therefore, this error measure may tap the contribution to biasing error made by low economic development in either or both nations in a dyad.

TABLE 4. GEOMETRIC MEANS OF BASIC INDICATORS ON FOUR SAMPLES

Basic	Geometric Mean of Loadings on Four Samples[a]
Tourists A→B	.65
Emigrants A→B/A's population	.72
Weighted U.N. voting distance A↔B	.84
Students A→B/A's students to all	.74
Exports A→B/A's GNP	.76
IGO A↔B/A's IGO membership	.79
Military Violence factor score A→B	.62
Embassy or Legation A→B/A's total Embassies and Legations	.74
U.N. voting distance on "Self Determination" pattern A↔B	.70
Anti-foreign behavior factor score A→B	.55

[a] Samples are raw selected, raw random, transformed selected, and transformed random. The geometric mean is computed for a basic indicator only on the four matching patterns for the four samples.

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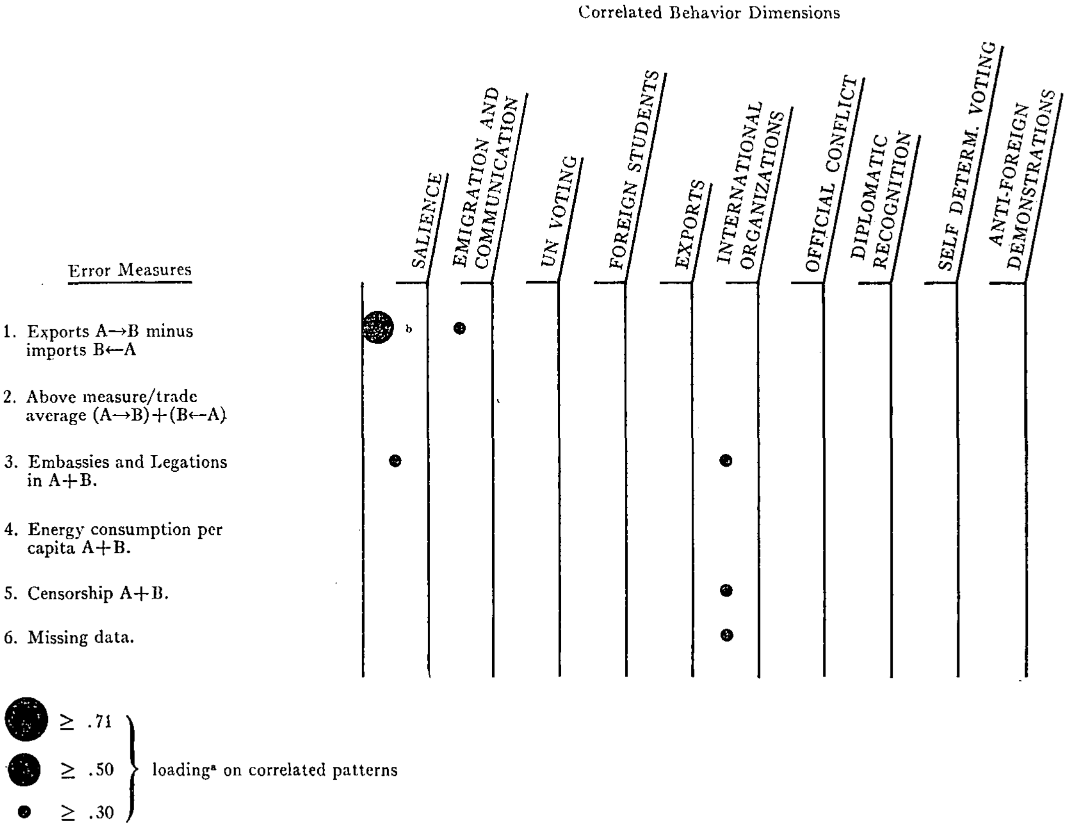
mantic (determined largely by one roll call as to whether the word "man" should be replaced by "human being" in the covenant of human rights), feminine rights, petitioning (Ethiopia), and self-government (Togo).

[46] R. J. Rummel, "Some Attribute and Behavioral Patterns of Nations," *Journal of Peace Research*, 2 (1967), 196–206.

[47] They were violent acts, planned violent acts, incidences of violence, discrete military acts or clashes, days of violence, negative acts, diplomatic rebuffs, negative communications, written negative communications, oral negative communications, written or oral negative communications, unclassified negative communications, accusations, representations or protests, warnings, and anti-foreign demonstrations.

FIG. 9. Loadings of Systematic Error Measures.



[a] Primary pattern matrix
[b] Loading of .55

5) Censorship A and B.
6) Missing data.

Figure 9 gives the relationship of the error measures to the ten behavioral patterns. The loadings are generally low, with the highest being .55. These overall results imply that the behavioral dimensions are largely statistically independent of systematic error *as tapped* by the error measures.

As with the attribute measures, we can ask: How correlated are the basic indicators of nation behavior? Table 5 shows the coefficient of determination between the basic measures. The highest coefficient is .10 and most coefficients are zero. This set of basic measures thus forms a nearly uncorrelated selection of indicators spanning the diverse behavior of nations and indexing the basic patterns involved.

IV. THE BASIC INDICATORS

The value of this set of major attribute and behavior concepts—patterns—and their basic indicators may not be fully appreciated, at first. Since these major concepts have been empirically delineated by factor analysis, the patterns they define span and embody variation on diverse national attributes and behavior.[48] A set of indicators of such patterns imply that *attributes or behavioral variables not in the set of basic indicators can be considered a (linear) combination of those in the set*. Therefore, with the set of basic indicators one should be able to predict or generate the data of nations on most of their other attributes or behavior.

Consider the total trade of a nation, for example. This is a nation's attribute of much interest in empirical and theoretical research. The variation of nations on this attribute is largely reflected, however, by their variation on the basic indicators, specifically energy consumption per capita and population. That is, the value of nation A on its total trade is[49]

$$\text{trade A} = .57 \text{ (A's energy consumption per capita)} + .75 \text{ (A's population)}.$$

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[48] In the terminology of linear algebra, the patterns (as factor dimensions) can be considered as bases of nation attribute and behavioral spaces.

[49] For all following examples, the data are assumed to be standardized.

TABLE 5. COEFFICIENT OF DETERMINATION BETWEEN BASIC INDICATORS*

Basic Indicators	1	2	3	4	5	6	7	8	9	10
1. Tourists A→B		111	90	108	123	123	123	123	90	123
2. Emigrants A→B/A's popul.	.02		100	111	131	131	131	131	100	131
3. Weighted U.N. voting dist.	.01	.01		100	132	132	132	132	132	132
4. Students A→B/A's stud.	.08	.08	.00		139	139	139	139	100	139
5. Exports A→B/A's GNP	.08	.07	.01	.01		182	182	182	132	182
6. IGO A⟷B/A's IGO mem.	.02	.02	.02	.00	.00		182	182	132	182
7. Military violence A→B	.00	.00	.06	.00	.00	.00		182	132	182
8. Emb. or Leg. A–B/A's total	.00	.00	.00	.00	.01	.00	.00		132	182
9. U.N. voting on "Self Determ."	.00	.00	.10	.00	.00	.00	.00	.00		132
10. Anti-foreign dem. A→B	.00	.00	.03	.00	.00	.00	.02	.00	.00	

* See the footnote to Table 2. Data are for the raw selected sample.

This equation accounts for about ninety percent of the variation of nations in their trade.[50]

Another interesting attribute is the number of intergovernmental organizations (IGO) of which a nation is a member. The variation of nations on this attribute is more complex than that for trade. For nation A,

A's IGO membership = .57 (A's energy consumption per capita) + .51 (A's bloc membership) + .33 (A's population) — .33 (A's Roman Catholics/population).

This combination of four basic variables should account for about eighty percent of the variation in IGO memberships among nations.

Measurements of communication attributes play a large role in some current theoretical work. Most communication attributes, however, can be expressed as a linear combination of energy consumption per capita. For example, for nation A,

A's newspaper circulation per cap. = .88 (A's energy cons. per cap.),
A's newsprint consumption per cap. = .89 (A's energy cons. per cap.),
A's literacy rate = .89 (A's energy cons. per cap.),
A's telephones per cap. = .95 (A's energy cons. per cap.),
A's domestic mail per cap. = .86 (A's energy cons. per cap.),
A's radio receivers per cap. = .91 (A's energy cons. per cap.).

The smallest variation energy consumption per capita accounts for among these communication variables in about seventy-seven percent (newspaper circulation). The highest variance accounted for is approximately ninety percent (telephones per capita). Since only one basic indicator is involved, communication attributes are proportional to energy consumption per capita and this *basic variable may be used alone as an indicator of the internal communications of nations*.

I have so far exemplified the meaning of the attribute basic indicators. Similar examples can be given for the behavior indicators. Most behavior between two nations should be a (linear) combination of the basic behavior indicators.

Consider treaties, for example. The number of treaties between two nations, A and B, should be,

treaties A ↔ B = .61 (tourists A → B) + .55 (emigrants A → B/A's population),

and accounting for about sixty-seven percent of the variance in treaties.

Mail and exports between nations have become important measures in theories of international integration. Variance in both kinds of behavior are represented by the behavior basic measures. To wit,

Mail A → B = .52 (tourists A → B) + .78 (emigrants A → B / A's total emigrants),
Exports A → B = .62 (tourists A → B) + .36 (exports A → B / A's GNP)

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[50] These and subsequent linear combinations (regressions) are derived from the orthogonal factor results of the 236 variable attribute and 40 variable (raw selected sample) behavioral analyses (Rummel, *Dimensions of Nations, op. cit.*). The values for the constants in the equations are the loadings of the dependent variable on the factor patterns indexed by the basic indicators.

+ .40 (students A → B / A's total students).

The variance accounted for is around eighty-eight percent for mail and about sixty-seven percent for exports.

One final example might be given. The private interaction between two nations, as opposed to public or official behavior, can be partly measured by their joint memberships in nongovernmental international organizations (NGO). The variation in co-memberships among pairs of nations is tapped by the basic behavior indicators,

NGO comemberships A ↔ B
= .54 (tourists A → B)
+ .34 (emigrants A → B / A's emigrants)
+ .53 (intergovernmental organization comemberships A ↔ B / A's total memberships),

for around sixty-nine percent of the variance in NGO co-memberships.

In summary, we can say that if Y is an attribute of nations, then for any nation i

$$Y_i = \alpha_1 X_{i1} + \alpha_2 X_{i2} + \ldots + \alpha_7 X_{i7},$$

where X is one of the seven basic attribute indicators. Similarly for internation behavior, if W is a behavior of nation i to nation j, then

$$W_{ij} = \alpha_1 Z_{ij1} + \alpha_2 Z_{ij2} + \ldots + \alpha_{10} Z_{ij10},$$

where Z is one of the basic behavior indicators.

I have focused so far on the ability of the basic indicators to account for the variance of nations on their attributes and behavior. There is another implication to the set of basic indicators at least as important. Since each basic indicator indexes a pattern of variation for nations, each basic measure indexes a functional unity—a cluster of phenomena that vary together.

Functional unities may represent causal influences within or between nations. Covariation of phenomena is a necessary but not sufficient requirement for their being a common cause. Therefore, we can consider the basic indicators as not only indexing major empirical concepts, but also we can view them as possible indicators of primary causal influences. This means that the basic indicators can be used to investigate the nature of such influences and represent them in causal analyses.

V. CONCLUSION

I stated in the beginning that by fastening major concepts to major patterns of variation in the empirical attributes and interactions of nations, this paper was meant to help refocus the analytic conceptual scheme employed in the study of international relations. A second and primary goal was to help define the best indicators of these concepts. These indicators have been described in the previous pages and there is no need to list them again. What has been accomplished in refocusing a conceptual scheme of international relations may be more subtle and require some elaboration here.

First, I have divided international relations onto two analytically distinct kinds of data: those about nation attributes and those about behavior between nations. Although hardly a new idea, what gives the distinction power is defining these two kinds of data by two matrices: one that is nation by attributes, and another is dyad (A→B) by behavior, respectively. These matrices are then conceived of as attribute and behavior *spaces;* each space comprises, respectively, all the interrelationships among the attributes and behavior of nations.[51] I have then

[51] The reason for dividing international relations into these two spaces is to mathematically resolve the problem of linking the characteristics of nations to their international behavior, or to use more fashionable language, to mathematically couple national subsystems to the international system. The representation of national attributes as a mathematical (Euclidean) space enables us to define nations as points in this space. Then we can define a distance vector between any two nation-points on each coordinate, say economic development, of space. Such distance vectors for all the coordinants of the space will measure the differences and similarities of nations across all their attributes.

Now, in the space of behavior we can locate pairs of nations—dyads—as points according to the behavior of the actor nation to the object nation. If we draw a line from the origin of this space to each dyad and add an arrowhead at the point where the line makes contact with the dyad, we then have a vector representation of each dyad's location in behavior space. The basic linkage between attribute space (nation attributes) and behavior space (nation behavior) is made through the distance vector. The magnitude and direction of a dyad vector in behavior space is a mathematical function of the distance vectors between the two nations in attribute space. That is

$$\vec{\omega}_{A\to B} = \sum_{i=1}^{p} \alpha_i \vec{d}_{A-Bj,i},$$

where $\vec{\omega}_{A\to B}$ = the vector describing the location of the dyad, $A\to B$, in behavior space, employed mathematics to locate the largest independent patterns of relationships in each space. Each pattern thus defined is an independent empirical concept maximally discriminating between attributes or between behavior. Economic development, size, and political orientation are three such major attribute patterns that have been found. Three such major patterns of behavior are U.N. voting, emigration and communications, and official conflict behavior.

Thus, empirical concepts, such as size and economic development, have been locked into a framework involving two data matrices, mathematical procedures for their analysis, and a theoretically fruitful mathematical representation of international relations. The refocusing which was my goal has thus involved picturing international relations through a wide angle mathematical lens that filtered out all but the distinct clusters of interrelated phenomena.

α_i = parameters for a dyad, $A \rightarrow B$,
$\vec{d}_{A-B,i}$ = distance vector between nation A and B in attribute space on coordinant (dimension) i.

Thus, international behavior is made a function of the similarities and differences between nations. The mathematical notions are fully integrated into one model, are operationalizable and the linkage is testable. The purpose of this paper, however, was not to present this linkage but rather a set of indicators (which locate each nation on the coordinants of these spaces). Describing this theory in detail would take us far afield from our original purpose. However, those wishing to read more on this theory, which I call a social field theory, are referred to my "A Field Theory of Social Action with Application to Conflict Within Nations," *General Systems Yearbook,* 10 (1965), 183–211; "A Social Field Theory of Foreign Conflict Behavior," *Peace Research Society: Papers, IV,* Cracow Conference, 1965, pp. 131–150; *Dimensions of Nations, op. cit.,* Chapter 16.

FOREIGN POLICY AS AN ISSUE AREA: A ROLL CALL ANALYSIS*

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I. INTRODUCTION

James N. Rosenau has elaborated the issue area typology developed by Theodore Lowi in an ambitious attempt to categorize motivations, role structures, and interaction sequences which distinguish "domestic" from "foreign" issues. Lowi classified domestic issues into three areas: distribution of resources, regulation of resources, and redistribution of resources, and he equated interaction sequences in these areas with the coalition (distributive), pluralist (regulatory), and elitist (redistributive) models of the political process. The theoretical underpinning of Lowi's typology was the assumption that stable expectations develop about appropriate patterns of political competition and conflict for issues in each of these three "arenas."[1]

The internal logic of Lowi's argument is as follows: (1) people's expectations concerning benefits to be derived from relating to others determine their choices of relationships; (2) governmental policies (outputs) determine expectations about questions of politics; (3) ergo, the type of policy at stake determines the patterning of any political relationship; a distinctive type of political relationship should characterize every major type of policy.[2] In these arenas of power, "each arena tends to develop its own characteristic political structure, political process, elites, and group relations."[3] Distributive policies are easily disaggregable into small units and can be apportioned among participants in relative isolation from one another. Regulatory policies, while specific in their impact, are not so infinitely disaggregable: "regulatory policies are distinguishable from distributive in that in the short run the regulatory decision involves a direct choice as to who will be indulged and who deprived."[4] Redistributive policies are similar to regulatory policies insofar as individual decisions are interrelated and apply to large categories of participants, but the impact of redistributive policies on these aggregates is much greater.[5]

Rosenau uses the Lowi formulations as a foundation for the development of hypotheses about expectations and interaction sequences characteristic of "domestic" as opposed to "foreign" issues.[6] The merit of Rosenau's ambition is that he attempts to partial out, theoretically, a distinction between these types of issue areas which is more than an exercise in analytical arbitrariness. His notion is that the common usage "foreign-domestic" dichotomy, if it is to be made fruitful for the development of hypotheses about the process of foreign policy-making, is best conceived as superimposed on Lowi's continuum of distributive-regulatory-redistributive types of policies. Or, it might be said that Rosenau conceives of the "domestic-foreign" continuum and the "distributive-regulatory-redistributive" continuum as intersecting planes, each plane subsuming characteristic behavior patterns dependent on expectations about: (1) the amount of available resources for whose allocation participants are competing; (2) the degree to which allocation of resources to one group or some groups necessarily entails deprivation of other groups.

On this basis, Rosenau suggests that the *way in which issues are processed* is determined by the extent to which processing of the issue involves perceived allocation of finite, limited societal resources. That is, if we take the subset of issues which, on the basis of subject-heading alone, we might intuitively classify as "foreign" (those which, following Samuel P. Huntington's distinction, involve to some extent allocations of values and resources in the international system

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[1] Theodore Lowi, "American Business, Public Policy, Case Studies, and Political Theory," *World Politics,* 16 (July, 1964), 677–715.

[2] *Ibid.,* p. 688.

[3] *Ibid.,* p. 690.

[4] *Ibid.,* p. 691.

[5] *Ibid.,* p. 691.

[6] James N. Rosenau, ed., *Domestic Sources of Foreign Policy* (New York: Free Press, 1967); Chapter I, Rosenau, "Foreign Policy as an Issue Area."

as well as in domestic politics),[7] some "foreign" policy issues might well be perceived as more redistributive than others, if they involve significant reallocations of finite national resources among fairly large national political groups.

It would seem, though, that not all actors in a given national political system would perceive the same substantive issues as falling within the same process typology categories of Lowi. It is also possible that important aggregates of actors might perceive many issues of "domestic" or "foreign" nature to fall in the same area of the Lowi distributive-redistributive continuum. For example, groups might perceive that the intersection of planes mentioned earlier occurs in such a fashion that all "domestic" issues fall into the "redistributive" sector of the Lowi continuum, regardless of subject matter (agriculture versus welfare, etc.). As an illustration, Lowi argues persuasively that issues relevant to the tariff were at one time perceived by major participants as falling on a "distributive" dimension, but later most participants exhibited behavior and perceptions which indicated that they ordered tariff questions within the "regulatory" dimension.

It might be useful to ascertain whether, at the aggregate level, significant groups of actors seem to view "foreign policy" issues much more frequently and consistently as distributive, regulatory, or redistributive, insofar as those categories embrace a continuum of perceptions from positive sum to zero sum. While the Bauer, Pool, and Dexter analysis in *American Business and Public Policy*[8] provides interesting data and represents one way to approach this question, the book is a case study of a single issue and generalizations beyond that case might require different lines of analysis. One such line of analysis constitutes the approach taken in this paper.

II. APPROACH AND METHODS

The United States Senate, 87th Congress, First and Second Sessions, constituted the population under study. A set of 115 roll calls on foreign policy issues in that body was subjected to techniques of cluster and factor analysis, the object of which was the inductive specification of content dimensions along which these roll call votes might cluster. Review of most of the literature in which techniques of cluster, scale and factor analysis are employed reveals that their use in the past has been designed to satisfy somewhat different purposes. Often the interest of the analyst has been in categorizing and explaining the behavior of different groups in legislative bodies and the degrees of cohesion or disagreement found to characterize these groups.[9]

As Bruce M. Russett and others have pointed out, factor analysis is basically a data reduction technique; it pivots on the assumption that a group of variables highly correlated among themselves, but which exhibit relatively low correlations with other variables, point to a principal underlying dimension, or factor.[10] Factor analysis

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[7] Samuel P. Huntington, *The Common Defense* (New York: Columbia University Press, 1960), pp. 1–10.

[8] Raymond Bauer, Ithiel de Sola Pool, and Lewis A. Dexter, *American Business and Public Policy: The Politics of Foreign Trade* (New York: Atherton Press, 1963).

[9] Summaries of the contributions of these sources may be found in Duncan MacRae, Jr., "A Method for Identifying Issues and Factions from Legislative Votes," this REVIEW, 59 (December 1965), 909–926; Bruce M. Russett, "Discovering Voting Groups in the United Nations," *ibid.*, 60 (June, 1966), 327–339; and Hayward R. Alker, Jr., "Dimensions of Conflict in the General Assembly," *ibid.*, 58 (September, 1964), 642–657. See especially, in addition to the above, Lee F. Anderson *et. al.*, *Legislative Roll-Call Analysis* (Evanston: Northwestern University Press, 1966); John G. Grumm, "A Factor Analysis of Legislative Behavior," *Midwest Journal of Political Science*, 7 (November, 1963), 336–356; L. N. Rieselbach, "The Demography of the Congressional Vote on Foreign Aid, 1939–1958," this REVIEW, 58 (September, 1964), 577–588; and Aage R. Clausen, *Policy Dimensions of Congressional Roll Calls* (unpublished doctoral dissertation, University of Michigan, 1964). Only results of factor analysis will be reported here. Comparison of results using factor analysis and cluster analysis is proceeding with the assistance of Aage R. Clausen and Richard B. Cheney.

[10] Alternative solutions of the correlation matrix according to the factor analytic model are possible. The choice of a principal components solution with orthogonal factors was made to facilitate interpretation, but the disadvantages of such a choice are noted by MacRae, *op. cit.* The principal components method of factor analysis is derived in Harry Harmon, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1960). Orthogonal factors are selected by Russett, *op. cit.*, and Gerald Marwell, "Party, Region and the Dimensions of Conflict in the House of Representatives, 1949–1954," this REVIEW, 61 (June, 1967), 380–399. MacRae discusses Marwell's approach and findings in "Partisanship and Issues in Congressional Voting," Paper delivered at the 1968 Annual Meeting of the

LISTING OF VARIABLES

V1 = Nomination of director, Export-Import Bank
V2 = Nomination of director, Export-Import Bank
V3 = Convention Creating Organization for Economic Cooperation and Development
V4 = Eliminate payments to Commodity Credit Corporation for sale of agricultural commodities for foreign currencies
V5 = Deny quota bonus on sugar to Dominican Republic as result of cutback on Cuba
V6 = Require Secretary of Labor to recommend quotas, tariffs on imports from low wage countries
V7 = Confirmation of ambassador to Iran
V8 = Amend Battle Act, bar aid to Communist states in debt to United Nations
V9 = Amend Battle Act, give President authority to give aid to countries other than USSR and Communist Far East
V10 = Inter-American Social and Economic Cooperation Program; Chilean Reconstruction
V11 = Recommit Educational and Cultural Exchange Act to committee
V12 = Members of Commission on International Education and Cultural Affairs be appointed on nonpartisan basis
V13 = Eliminate travel expenses for dependents of participants in cultural exchange
V14 = Finance foreign students in cultural exchange at U.S. schools only
V15 = Prohibit President from making agreements with international organizations to sponsor exchange programs
V16 = Loyalty questions and security risks, in cultural exchange programs, must be investigated by FBI
V17 = Extension of Alien Orphan Act
V18 = Extension of Alien Orphan Act
V19 = Cut funds for purchase of radiological defense equipment by the Office of Civil Defense Mobilization
V20 = Foreign aid. Require annual appropriation for Development Loan Fund
V21 = Foreign aid. Congress must approve loans over $5 million and President must submit such loan plans to Congress 30 days in advance
V22 = Foreign aid. Congress must approve DLF budget annually, and its approval required on loans over $5 million.
V23 = Reduce military assistance for FY 1962, 1963
V24 = Foreign aid. Reduce borrowing authorization for FY 1962–66
V25 = Foreign aid. Limit U.S. contribution to UN Congo operations to 40 per cent of total cost
V26 = Foreign aid. Set ceiling on development loan fund interest rate
V27 = Foreign aid. Reduce DLF borrowing authorization for FY 1963–66
V28 = Foreign aid. Reduce military assistance for FY 1962–1963
V29 = Foreign aid. Require that equipment or property not purchased in recipient country be purchased in U.S.
V30 = Foreign aid. End military grants to Europe, except for national emergency
V31 = Foreign aid. Reduce DLF borrowing authorization from five to three years
V32 = Foreign aid. Earmark 30 per cent of development loan funds for private enterprise
V33 = Foreign aid. Bar aid to any Communist-controlled countries
V34 = Foreign aid. Require grants over $5 million to be approved by Congress
V35 = Foreign aid. Bar aid to countries exporting arms and strategic materials to Soviet-controlled nations
V36 = Foreign aid. Deny DLF loans or grants to nations behind in UN assessments
V37 = Foreign aid. Control interest rates required by recipient country reloaning development loans
V38 = Foreign aid. Establish ceiling on interest rates for relending of DLF funds
V39 = Passage of Foreign Assistance Act
V40 = Peace Corps Act. Reduce authorization
V41 = Recommit nomination of Gen. Williams for promotion of Brigadier General in U.S. Army Reserves
V42 = Nomination of Gen. Williams
V43 = Increase funds for USIA informational media guarantee program
V44 = Conference report, FY 1961 Foreign Assistance Act
V45 = Establishment of Arms Control and Disarmament Agency. Amend to eliminate agency authority to conduct, support, or coordinate studies on disarmament
V46 = Establish Arms Control and Disarmament Agency. Send bill back to Armed Services Committee

LISTING OF VARIABLES—(*Continued*)

V47 = Cut military aid 10 per cent
V48 = Passage of Foreign Assistance Appropriation Act for FY 1962
V49 = Confirm McCone as CIA head
V50 = Withhold U.S. aid from UN members in arrears
V51 = Prohibit U.S. participation in UN military operations without Congressional approval, and relieve UN from repayment to the U.S. of debts incurred in Congo
V52 = U.S. can loan to UN, above $25 million, only amounts matched by loans of other UN members
V53 = Limit U.S. loans to UN
V54 = Authorize President to make three-year, $100 million loan to UN at interest rate U.S. treasury paid on its own borrowing
V55 = President authorized to loan up to $100 million to UN
V56 = Textiles and farm products, imports
V57 = Textiles and farm products, imports
V58 = Textiles and farm products, imports
V59 = Foreign Assistant Act of 1962
V60 = Foreign Assistance Act of 1962. Amend to permit President to sell or give surplus food to Communist countries such as Yugoslavia and Poland
V61 = Bar aid to any nation which exported to USSR, or any nation dominated by USSR, any war implements or strategic articles
V62 = Passage of Foreign Assistance Act of 1962
V63 = Department of Defense Appropriations. Provide funds for development of RS-70
V64 = Department of Defense Appropriations. Limit funds for RS-70
V65 = Sugar Act Amendments of 1962
V66 = Establish space program manpower commission
V67 = Nomination of ambassador to Ireland
V68 = Conference Report, Foreign Assistance Act of 1962
V69 = State Dept., rather than Comsat, should supervise or conduct relevant foreign agreements
V70 = Require President to make arrangements for determining UN role in satellite system of U.S.

proceeds by establishing product-moment correlations of every variable in the analysis with every other variable, and seeks to establish whether large proportions of those variables which are highly inter-correlated exhibit those relationships due to high common "loadings" on, or correlations with, a few significant dimensions or "factors."[11]

The data, as mentioned, consisted of 115 roll calls in the 87th Congress, Senate, which seemed to have any foreign and defense policy component. Initial measures of association among items were obtained by computing a Yule's Q correlation of the vote distribution on each roll call with every other roll call. After these data were obtained, elimination of those roll calls which approached unanimity (defined here as items on which the distribution was more one sided that 90–10 per cent) was facilitated. Elimination of such items from the analysis was necessary because the product-moment correlation coefficient, on which this factor analysis depends, is distorted by a voting distribution more lopsided than 90–10.[12] Fourteen roll calls were dropped from the analysis for this reason. Inspection of the matrix of Q-coefficients among the remaining items led to elimination from the analysis of those items which had the least number of members present and voting. Thus reduc-

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American Political Science Association, Washington, D.C., Sept. 2–7. See also Jeanne E. Gullahorn, "Multivariate Approaches in Survey Data Processing: Comparisons of Factor, Cluster, and Guttman Analyses and of Multiple Regression and Canonical Correlation Methods," in *Multivariate Behavioral Research Monographs,* No. 67-1, 1967, and D. M. Wilkins, *Factor Analysis and Multiple Scalogram Analysis: A Logical and Empirical Comparison* (unpublished doctoral dissertation, Michigan State University, 1962). For an interesting argument in favor of the use of "elementary" factor analysis, as opposed to standard Q and R analysis, see Ernest A. Chaples, Jr., *The Voting Behavior of United States Senators for Four Selected Issues, 1953–1964* (unpublished doctoral dissertation, University of Kentucky, 1967).

[11] For an interesting application of factor analysis to a problem involving relatively few variables, which makes clear some of the assumptions and problems associated with use of this technique, see Joseph A. Kahl and James A. Davis, "A Comparison of Indexes of Socio-Economic Status," *American Sociological Review,* 20 (June, 1955), 317–325.

[12] On this point see Russett, *op. cit.,* p. 329.

TABLE 1. LOADINGS, RAW FACTOR MATRIX

Variable	Factor 1	Factor 2	Factor 3
1	.03	.40	.52
2	.04	−.41	−.46
3	.49	.39	−.02
4	−.60	.69	−.09
5	.10	.02	−.41
6	−.75	−.01	−.04
7	.57	−.36	.06
8	−.72	.08	.03
9	.75	.22	.03
10	.68	−.25	−.14
11	−.73	.18	−.00
12	.80	−.30	.07
13	−.81	.16	−.05
14	−.82	.20	−.12
15	−.84	−.12	.01
16	−.62	.18	−.19
17	.33	−.62	−.19
18	.20	.67	.10
19	−.34	.25	.47
20	−.85	−.18	−.07
21	.72	.24	−.20
22	.69	−.05	−.16
23	−.42	−.47	.17
24	−.83	−.10	−.04
25	−.81	−.27	−.00
26	.70	−.40	−.20
27	−.81	−.03	.01
28	−.46	−.20	.27
29	−.84	−.00	−.02
30	−.07	−.42	.54
31	−.88	.03	−.10
32	−.80	−.17	−.01
33	.44	.44	.00
34	−.81	.29	−.09
35	−.83	.00	−.01
36	−.78	.08	−.00
37	.71	−.16	−.23
38	−.33	.14	.14
39	.71	.53	−.05
40	−.80	.23	−.08
41	−.55	.61	−.01
42	.55	−.61	.01
43	.26	.49	.20
44	.70	.53	−.06
45	−.74	−.05	−.20
46	−.74	−.01	−.12
47	−.45	−.33	.29
48	.63	.55	−.04
49	−.15	−.08	−.59
50	−.56	−.23	.14
51	−.71	−.22	.01
52	−.73	−.15	.10
53	−.82	−.08	−.00
54	−.64	.26	.00
55	.73	.31	−.05
56	−.70	.44	−.11
57	.67	−.44	.10
58	.56	−.52	.03
59	−.48	−.33	.09
60	.59	.41	−.07
61	−.85	.05	.01
62	.66	.55	−.09
63	.15	−.06	.47
64	−.11	.14	.30
65	.10	−.22	−.46
66	.27	.10	.56
67	−.62	.66	−.08
68	.68	.40	−.23
69	.33	.05	.67
70	−.32	−.03	−.55

tion of the number of roll call items to the maximum number permitted by the factor analysis program was accomplished. Each legislator was coded on every roll call as either affirmative, abstaining, or negative, with affirmative and negative as upper and lower boundaries and abstention as intermediate.

One unfortunate outcome is that roll calls with a more lopsided vote distribution (in terms of yeas and nays) than 90–10 per cent may be of substantive interest in their own right. Here, there is less choice for the analyst methodologically, for one simply cannot include such items in a factor analysis, based upon a matrix of product-moment correlations, without danger of distortion. But the result is that certain items eliminated by the criterion of "too close to unanimity" might be of interest to the theorist, such as votes on military appropriations for the Defense Department. The same can be said for roll calls eliminated according to the yardstick of least number of legislators present and voting.

All of this reminds us of an important consideration relevant to interpretation of all these results, and similar roll call analyses: roll calls are only partial, and very partial, subsets of data relevant to the issue of Congressional perceptions and orientations on foreign policy issues. They do not capture the bargaining in committees, negotiations on the floor, or powers of persuasion and definition exercised by the executive. They are poor ersatz for extensive interview data, in some cases. Thus any analysis of roll call data is likely to generate inferences which must remain close to the vest of the researcher. Nonetheless, specification of dimensions in roll call data may suggest aggregate level relationships which assist us in framing

TABLE 2. LOADINGS, ROTATED FACTOR MATRIX

Variable	Factor 1	Factor 2	Factor 3
1	.11	.13	−.01
2	−.19	−.07	−.00
3	−.06	.71	−.13
4	.82	.09	.32
5	−.02	.11	−.03
6	.43	−.43	.46
7	−.64	.16	−.29
8	.45	−.30	.41
9	−.20	.43	−.65
10	−.46	.18	−.40
11	.69	−.38	.17
12	−.66	.17	−.50
13	.42	−.27	.70
14	.47	−.20	.71
15	.36	−.60	.55
16	.36	−.12	.54
17	−.61	−.06	−.12
18	.49	.45	−.30
19	.31	.00	−.00
20	.33	−.50	.54
21	−.20	.51	−.37
22	−.45	.32	−.26
23	−.08	−.34	.22
24	.33	−.41	.63
25	.21	−.48	.57
26	−.55	.05	−.45
27	.30	−.31	.71
28	.05	−.16	.35
29	.54	−.51	.34
30	−.23	−.44	−.06
31	.42	−.40	.65
32	.33	−.50	.38
33	.10	.48	−.39
34	.67	−.23	.40
35	.32	−.33	.72
36	.54	−.36	.29
37	−.36	.23	−.52
38	.13	−.05	.25
39	−.11	.87	−.22
40	.55	−.12	.56
41	.78	.05	.25
42	−.78	−.05	−.25
43	.10	.32	−.14
44	−.10	.86	−.23
45	.27	−.25	.68
46	.34	−.21	.55
47	.10	−.47	.06
48	−.05	.89	−.16
49	.07	−.07	.04
50	.07	−.36	.20
51	.27	−.52	.19
52	.29	−.42	.30
53	.44	−.46	.30
54	.67	−.25	.14
55	−.20	.61	−.25
56	.87	−.16	.18
57	−.86	.15	−.16
58	−.81	.07	−.04
59	.03	−.43	.22
60	.01	.59	−.36
61	.45	−.43	.53
62	−.04	.83	−.22
63	−.18	.09	.04
64	.12	.00	.03
65	−.15	.00	.03
66	−.07	.05	−.17
67	.85	.03	.24
68	−.14	.76	−.20
69	−.11	.12	−.20
70	.12	−.09	.15

more specific questions, or which may be of intrinsic interest.

In Table 1, the factor "loadings" of every variable (roll call) on the factors in the raw factor matrix are presented. The program included a normalized varimax rotation of the original factor matrix. Russett has described concisely the purposes of such rotation:

> 'Rotating' the original factors to 'simple structures' maximizes the number of both very high and very low loadings, thus making interpretation easier.[13]

Thirteen factors corresponding to eigen values of the correlation matrix which were greater than one were extracted and rotated. Three of these factors seemed important for substantive interpretation: factor 1 accounted for 18 per cent of total variance, factor 2 for 16 per cent, and factor 3 for 14 per cent. Summary of some of the more important results follows in tabular form.

Substantive interpretations of dimensions resulting from factor analyses can be controversial, especially when the matrix of correlations grows to the size used in this study. Some of this difficulty might be avoided if we could determine, for each observation, both (1) the factor on which it recorded its highest relative loading, and (2) its position within a rank ordering of observations which loaded highest on each factor. Table 3 reports these rankings for loadings of variables in the rotated factor matrix, for all those variables with loadings greater than one-half.[14]

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[13] Russett, *op. cit.*, p. 331.

[14] This criterion is incorporated into the program

TABLE 3. RANKS OF LOADINGS FOR THE ROTATED FACTOR MATRIX

Factor 1

1. Textiles and farm products, imports
2. Textiles and farm products, imports
3. Nominate ambassador to Ireland
4. Eliminate payments to Commodity Credit Corporation for sale of agricultural commodities for foreign currencies
5. Textiles and farm products, imports
6. Recommit nomination of Gen. Williams for promotion in U.S. Army Reserves
7. Nomination of Gen. Williams
8. Education and Cultural Exchange Act
9. Authorize President to make loan to UN
10. Foreign aid. Require grants over $5 million to be approved by Congress
11. Members of Commission on International Education and Cultural Affairs be appointed on non-partisan basis
12. Confirm ambassador to Iran
13. Extension of Alien Orphan Act
14. Reduce authorization for Peace Corps
15. Set ceiling on Development Loan Fund interest rate
16. Foreign aid. Require that equipment or property not purchased in recipient country be purchased in U.S.
17. Foreign aid. Deny DLF loans or grants to nations behind in UN assessments

Factor 2

1. Passage of Foreign Assistance Appropriation Act for FY 1962
2. Passage of Foreign Assistance Act of 1961
3. Conference Report, FY 1961 Foreign Assistance Act
4. Passage of Foreign Assistance Act of 1962
5. Conference Report, Foreign Assistance Act of 1962
6. Convention creating Organization for Economic Cooperation and Development
7. President authorized to loan $100 million to UN
8. Prohibit President to make agreements with international organizations to sponsor cultural exchange programs
9. Foreign Assistance Act of 1962. Amend to permit President to sell or give surplus food to Communist countries such as Yugoslavia or Poland
10. Prohibit U.S. participation in UN military operations without Congressional approval
11. Foreign aid. Congress must approve loans over $5 million and President must submit such loan plans to Congress 30 days in advance
12. Foreign aid. Require that equipment or property not purchased in recipient country be purchased in U.S.
13. Foreign aid. Require annual appropriation for Development Loan Fund
14. Foreign aid. Earmark 30 per cent of development loan funds for private enterprise

Factor 3

1. Foreign aid. Bar aid to countries exporting arms and strategic materials to Soviet-controlled nations
2. Foreign aid. Reduce Development Loan Fund borrowing authorization for FY 1963–66
3. Finance foreign students in cultural exchange at U.S. schools only
4. Eliminate travel expenses for dependents of participants in cultural exchange
5. Establishment of Arms Control and Disarmament Agency. Amend to limit agency authority
6. Foreign aid. Reduce DLF borrowing authorization from five to three years
7. Amend Battle Act, give President authority to give aid to countries other than USSR and Communist Far East
8. Foreign aid. Reduce borrowing authorization for FY 1962–66
9. Foreign aid. Limit U.S. contribution to UN Congo operations to 40 per cent of total cost
10. Inter-American Social and Economic Cooperation
11. Establish Arms Control and Disarmament Agency. Send bill back to Armed Services Committee

TABLE 3—(*Continued*)

12. Prohibit President from making agreements with international organizations to sponsor cultural exchange programs
13. Foreign aid. Require annual appropriation for Development Loan Fund
14. Loyalty questions and security risks, in cultural exchange programs, must be investigated by FBI
15. Bar aid to any nation which exported to USSR, or any nation dominated by USSR, any war implements or strategic articles
16. Foreign aid. Control interest rates required by recipient country reloaning development loans
17. Membership of Commission on International Education and Cultural Affairs

Seventeen variables (roll calls) met this criterion for factor 1. Generally, they represent attempts by senators, through amendments to a variety of substantive bills, to reduce aid appropriations and restrict Presidential prerogatives in dispensing with funds to finance programs of agencies which operate overseas. It is interesting to observe that this factor cuts across a number of substantive measures, from the Peace Corps, to Development Loan Fund, to textile and farm imports, to Foreign Assistance Act, to Educational and Cultural Exchange Act, and so on. Loadings on this factor range from a high of .87 (textiles and farm product imports) to a low of .54 (deny Development Loan Fund aid to nations defaulting UN assessments), among these seventeen items.

On factor 2, fourteen items registered loadings meeting the one-half criterion. It is more difficult to discern a consistent pattern in this factor other than one very obvious tendency: the factor picked up items representing final passage and approval of a variety of substantive measures, including the Foreign Assistance Act of 1961, the Convention creating the Organization for Economic Cooperation and Development, the Foreign Assistance Act of 1962, U.S. aid to the United Nations, and so on. Initial suspicions were that this merely represented a "general acquiescence" phenomenon: the battles over amendment and committee squabbles having been concluded by the time the final vote is taken, senators are in widespread agreement to push a bill to passage. But, recall that all roll calls which approached too closely to unanimity were eliminated from the analysis. Considering this, it is more likely that this factor might represent the relatively stable alignments, associated with positions favoring or opposed to additional foreign aid, as they persisted through to the final "approve or disapprove" voting on the measures in their entirety.

Factor 3 contains echoes from factor 1, but seems more specifically referent to foreign aid per se. The high rank ordering of the amendment to limit the authority of the Arms Control and Disarmament Agency, for example, is redolent of efforts to limit agency prerogatives which marked factor 1.

III. INTERPRETATIONS AND CONCLUSIONS

The patterns in these data suggest the following inferences.

1. The primary dimension emerging from this analysis seems to be a tendency on the part of senators in the 87th Congress to restrict American economic spending abroad and to limit Presidential prerogatives in this respect. This suggests a generalized orientation to these foreign policy issues of "guardians of the public purse," perhaps related to views of these programs as competing with "domestic" programs for (what are felt to be) limited resources. Factor analysis of foreign policy roll calls in the 87th Senate, then, detected a primary dimension representing attempts to reduce spending, and to limit economic prerogatives, largely in the substantive area of foreign economic assistance. We are not the first to observe that foreign aid issues, of the general area of all "foreign policy" issues, might be most similar to "domestic" issues in form and process.[15] Perhaps, in the subset of foreign policy issues probably closest to the "domestic" and most distant from the "foreign" poles of that continuum, a predominant pattern of redistributive orientations appears. This confirms to some extent the implications of Rosenau's extrapolation of Lowi's argument: *the more a foreign policy issue falls toward the "domestic" end of the domestic-foreign continuum, the more likely it is to be perceived as redistributive, as opposed to distributive.*

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used to perform the factor analysis, *Factor 1*, of the University of Wisconsin Computing Center.

[15] See, for example, the insightful study by Michael Kent O'Leary, *The Politics of American Foreign Aid* (New York: Atherton Press, 1967). O'Leary notes that Congress, pressure groups, and members of the general public often tend to react to foreign aid "in terms of domestic standards and expectations" associated with it, *ibid.*, p. 123.

2. Loose discussions about "isolationist" dimensions within Congress or "isolationist" Congresses may be misplaced. The pattern represented in these data supports claims (which find widespread support in the literature) that Congressmen often feel bewildered by the complexity of foreign affairs issues and would be unlikely to order their orientations on these issues in terms of a single strong ideological continuum. Instead, they are encouraged by the committee structure and fragmentation of duties to consider bills as discrete proposals, and are often motivated to view these bills, regardless of substance, in terms of profligacy or husbandry of government funds.

3. Cynical views of Congress often lead to the suspicion that senators' knowledge of foreign affairs, in many cases, is scant and that their preceptions of these issues, apart from cues by key leaders, are idiosyncratic. This may be the case, and I would not dispute that claim on the basis of my very partial data. Yet it seems, on the basis of the evidence presented here, that there may be latent if not manifest structure to these perceptions. Perhaps the Congressman tends to translate foreign issues into terms by which he orients himself on domestic issues—such as cut or not cut.

Final note must be made of the limitations of this type of analysis as applied to this type of problem. Factor analysis, as used in the fashion I have described, maximizes the possibility of producing order out of chaos, and in the process makes certain assumptions which might be "unrealistic." It is entirely possible that the "real" pattern may be chaos. As applied to foreign policy issue analysis, roll call data may be two steps removed from reality: (1) the executive monopolizes the expertise on much foreign policy decision making and may be the locus for most of the important decisions, with Congress merely exercising its right (and rite) of ratification; (2) roll call data, even for Congress, are only partial information on orientations toward these issues. Further, any attempt to operationalize the formulations of Rosenau, while in the process of extrapolating arguments of Lowi, with roll call data, raises the possibility of an enormous gap between theoretical and operational definitions of identifiable dimensions. The crudity of the data and the operations performed on them could never do complete justice to Rosenau's hypotheses, which require improved longitudinal analyses of this and other types before they can be either verified or dismissed.

INCOME INEQUALITY AND AMERICAN STATE POLITICS*

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The rapidly expanding research literature in comparative state politics typically measures its dependent and independent variables with references to means, averages, and per capitas for each state.[1] (For example, environmental inputs may be expressed as per capita personal income, percent of population living in urban places, median school year completed by the population over 25, etc.; political characteristics may be expressed as average voter turnout levels, percent of the total vote cast for the winning party, etc.; policy outputs are often expressed as per capita expenditures for education, average monthly payments for old age assistance, per capita tax revenues, etc.) With measures such as these, the comparative state politics research has systematically explored many of the linkages between environmental inputs, political system characteristics, and public policy outcomes.

Perhaps the most serious reservation regarding this research is its failure to examine distributive and redistributive aspects of state politics. Both dependent and independent variables are generally expressed as *levels* or *amounts* or *averages* for whole states; these can be neatly arranged for comparative analysis both longitudinally and cross-sectionally. But what about the *distribution* of wealth within a state? Or the *distribution* of public monies among high and low income groups, rural and urban populations, or other divisions within a state's population. The linkages between the distribution of resources within states, the distribution of influence within state political systems, and public policies reflecting distributional decisions, remain largely untested.

For example, scholars have explored the relationship between per capita personal income and levels of public expenditures and services in the 50 states.[2] Income consistently shows up as a very influential variable in determining expenditure levels of state and local governments, and also correlates nicely with party competition and voter turnout. But what is the effect of *inequality in the distribution of income* on politics and public policy within states? Certainly there are good *a priori* reasons for hypothesizing that the degree of income inequality in a state affects both the character of its political system and the content of its policies. One would reasonably expect to find political differences between a state in which wealth is concentrated in the hands of a few, and a state in which wealth is more widely distributed, even if the median incomes of these states are similar.

Fortunately, economists have come up with a way of measuring income distributions within political systems. Income distributions may be observed by means of a Lorenz curve which shows the cumulative proportions of aggregate income (on the vertical or y axis) accruing to cumulative proportions of the population ranging ing in order from the lowest to highest income-earners (on the horizontal or x axis).[3] In a political system with perfect income inequality, income would cumulate in direct relation to cumulated proportions of the population. Perfect equality is represented on a Lorenz chart by a diagonal straight line rising from the lower left-hand corner to the upper right-hand corner. But in real political systems, the lowest income-earners earn a smaller proportion of the aggregate income than their proportion of all income-earners. Therefore, when cumulative proportions of the population ranging from lowest to highest

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* I am grateful for the comments and suggestions of John Q. Wilson, Department of Economics, Yale University; Elliott Morss, International Monetary Fund; and Edmund P. Fowler, Department of Political Science, York University.

[1] See, for example Richard E. Dawson and James A. Robinson, "Interparty Competition, Economic Variables, and Welfare Policies in the American States," *Journal of Politics*, 25 (May, 1963), 265–289; Richard I. Hofferbert, "The Relationship Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW, 60 (March, 1966), 73–82; Ira Sharkansky, "Economic and Political Correlates of State Government Expenditures," *Midwest Journal of Political Science*, 11 (May, 1967) 173–192; John Crittenden, "Dimensions of Modernization in the American States," this REVIEW, 61 (December, 1967), 989–1001; Thomas R. Dye, *Politics, Economics and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally, 1966).

[2] See Footnote 1.

[3] M. C. Lorenz, "Methods of Measuring the Concentration of Wealth," *Publications of the American Statistical Association*, 9 (1905), 209–219; see also Mary J. Bowman, "A Graphical Analysis of Personal Income Distributions in the United States," *American Economic Review*, 35 (September, 1945), 618–628.

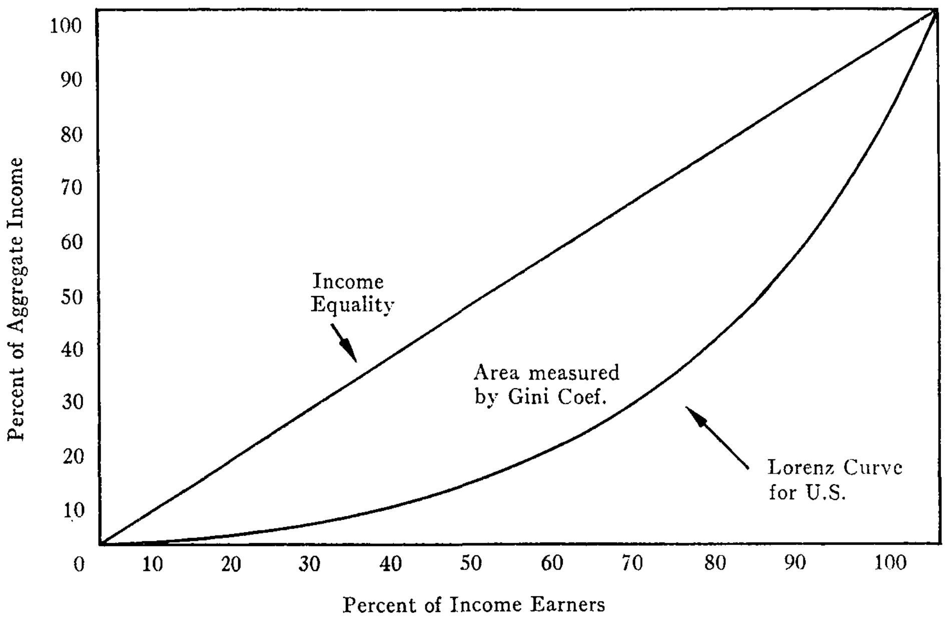

FIG. 1. The Lorenz Curve of Income Inequality.

income-earners are plotted against the proportion of aggregate income they actually receive, the result is a convex "Lorenz" curve. The greater the degree of convexity of this plotted curve, the greater the degree of inequality in the political system. The total area on a diagram which falls between the Lorenz curve, representing the actual income distribution, and the straight diagonal line, representing perfect income equality, expresses the extent of income inequality within a political system. This area is measured by a Gini coefficient or Gini index, which ranges from a +1.00 (theoretical perfect inequality) to 0.00 (theoretical perfect equality).[4]

Gini indices for the 50 states and the United States are presented in Table 1.[5] Income inequality is greatest in Mississippi and least in Utah. The utility of this measure is obvious: It provides a single comparable measure for each state and permits us to systematically explore linkages between income inequalities and other measurable political system and public policy variables.

But before we turn to an examination of the relationships between income distributions and public policy, it is important to observe the relationships between income distributions and other environmental variables in the states. Table 2 shows that income inequality is closely associated with levels of income, urbanization, industrialization, and adult education in the states. The lower a state's median income, the greater the income inequality. Inequality is also associated with ruralism, agriculturalism, and lower adult education levels.[6] These environ-

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[4] Corrado Gini, "On the Measure of Concentration with Especial Reference to Income and Wealth," Paper Delivered before the Cowles Commission, 1936; see also James Morgan "The Anatomy of Income Distributions," *The Review of Economics and Statistics,* 44 (August, 1962), 270–280.

[5] These Gini indices were computed by Thomas D. Hopkins from income distributional data for total families and unrelated individuals in 1959 from U. S. Bureau of the Census, *U. S. Census of Population: 1960* serves PC (1) 1C to 53C, "General Social and Economic Characteristics," Table 65. See Thomas D. Hopkins, "Income Distribution in Grants-in-Aid Equity Analysis," *National Tax Journal,* 18 (June, 1965), 209–213. Slightly different Gini indices were computed from the same source by Al-Samarrie and Miller which correlate .89 (product moment) with the indices above and produce substantially the same results. See Ahmad Al-Samarrie and Herman P. Miller, "State Differentials in Income Concentration," *American Economic Review,* 57 (March, 1967), 59–72.

[6] These findings parallel those of Al-Samarrie and Miller who undertook a comprehensive explanation of income inequalities in the states. They regressed Gini coefficients on ten independent variables, including property income as a percent of total

TABLE 1. INCOME DISTRIBUTIONAL INEQUALITY IN THE AMERICAN STATES

State	Gini Coefficient	State	Gini Coefficient
1. Mississippi	.510	26. Minnesota	.431
2. Arkansas	.486	27. North Dakota	.430
3. Alabama	.478	28. New York	.429
4. Tennessee	.478	29. California	.427
5. Louisiana	.477	30. Colorado	.425
6. South Carolina	.474	31. Maryland	.424
7. Kentucky	.474	32. Illinois	.423
8. Georgia	.469	33. Rhode Island	.418
9. Oklahoma	.465	34. Montana	.415
10. North Carolina	.465	35. Indiana	.414
11. Texas	.464	36. Nevada	.414
12. Florida	.462	37. Massachusetts	.414
13. Virginia	.461	38. Washington	.413
14. Missouri	.459	39. Pennsylvania	.412
15. South Dakota	.456	40. Wisconsin	.412
16. Alaska	.456	41. Maine	.412
17. West Virginia	.451	42. Oregon	.411
18. Hawaii	.446	43. Michigan	.409
19. Arizona	.445	44. Ohio	.408
20. New Mexico	.440	45. New Hampshire	.407
21. Nebraska	.440	46. Connecticut	.404
22. Kansas	.439	47. New Jersey	.403
23. Iowa	.439	48. Idaho	.402
24. Delaware	.434	49. Wyoming	.399
25. Vermont	.434	50. Utah	.394

U.S. as a whole: GINI = .439

mental variables and many policy outcomes in the fields of education, health, welfare, highways, and taxation.

Table 2 also shows the relationships between income distributions and two environmental factor scores derived by Richard I. Hofferbert.[7] These factor scores purport to show underlying dimensions of socio-economic variables in the states; Hofferbert employed factor analysis to identify two independent environmental dimensions which he labeled "Industrialization" and

TABLE 2. INCOME INEQUALITY AND ECONOMIC DEVELOPMENT IN THE STATES

Figures are simple correlation coefficients with Gini Index*

Variable	Coefficient	Variable	Coefficient
Per Capita Personal Income	−.64	Median School year completed	−.69
Median Family Income	−.77	Nonwhite Population	.72
Urbanization	−.50	Industrialization Factor Score	−.35
Industrialization	−.36	Cultural Enrichment Factor Score	−.67

* All correlation coefficients reported in this study are product moment and not rank order coefficients.

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personal income, agricultural earnings as a percent of total labor earnings, median school years completed by persons 25 years and over, percent nonwhite population, and employment as a percent of population. Percent non-white turned out to be their single most explanatory variable; they did not employ a measure of family income. See Ahmad Al-Samarrie and Herman P. Miller, "State Differentials in Income Concentration," *American Economic Review,* 57, (March, 1967), 59–72.

[7] Richard I. Hofferbert, "Socio-economic Dimensions of the American States: 1890–1960", *Midwest Journal of Political Science,* 12 (August, 1968), 401–418.

TABLE 3. RELATIONSHIPS BETWEEN INCOME INEQUALITY AND SELECTED POLITICAL SYSTEM VARIABLES IN THE AMERICAN STATES

		Compared to coefficients obtained with specific levels of:				factor scores for:	
Selected Political Variables	Coefficients Obtained with Gini Index	Income	Industrialization	Education	Urbanization	Industrialization	Cultural Enrichment
Party Competition							
Governor	−72*	66	18	61	29	22	63
Lower Houses	−61*	52	00	56	26	19	54
Upper Houses	−59*	51	03	49	30	23	52
Voter Participation							
Governor	−71*	52	05	49	17	18	53
Congress	−80*	66	09	63	26	19	60
Democratic Success							
Governor	66*	−56	02	−59	−22	−11	−60
Lower Houses	72*	−46	−10	−54	−05	−13	−52
Upper Houses	74*	−48	−03	−51	−15	−16	−51
Malapportionment							
Index of Rep.	06	−21	−19	−18	−24	−03	−21
Urban Under-rep.	−47*	36	33	06	27	54	05
Apportionm. score	−32*	14	15	13	00	19	06
Interest Group Strength	42*	−42	08	−25	−06	−15	−21
Length of Constitution	30*	−20	03	−28	05	−04	−24
Elected St. Agency Heads	43*	−53	−42	−32	−28	−41	−26
Governor's Power Index	−42	60*	38	37	44	37	36

* Highest simple coefficient among independent variables.

"Cultural enrichment."[8] All of Hofferbert's base variables are per capitas, means, or averages for whole states; they do not include distributional variables. Apparently income inequality is negatively associated with both the "industrialization" factor and the "cultural enrichment" factor; an increase in industrialization or cultural enrichment is associated with a reduction in income inequality.

Table 3 shows the relationships between income inequality and several common political system variables in the states.[9] One may compare the coefficients obtained with Gini index, with the coefficients obtained with specific socio-economic variables and with factor-analyzed socio-economic dimensions.

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[8] The composition of Hofferbert's factors can be observed in the following factor loadings: "Industrialization" — value mfg. .91, pop. in mfk. .88, farm value .83, density .78, foreign born .70, population .67, urban .66, telephones .65, number of employees .64, income .57, business failures .42, property .13, negro .07, illiteracy .04, pop. increase —.00, school years —.03, tenancy —.27, owner-occupied —.32, divorce —.33, acreage —.50, motor vehicles —.57; "cultural enrichment"—school years .91, property .79, income .73, motor vehicles .70, telephones .68, pop. increase .55, urban .52, acreage .49, divorce .43, business failures .29, owner-occupied .24, foreign-born .23, population .05, farm value .02, value mfg. .01, density .01, pop. in mfg. —.13, number of employees —.35, tenancy —.47, illiteracy —.74, negro —.75: *ibid.*

[9] These political system variables are: Competition: one minus the percentage of votes cast in Gubernatorial elections 1954 to 1964 for majority party, one minus the proportion of seats held by the majority party in the upper and lower chambers of the state legislature 1954 to 1964; voter participation: average voter turnout in gubernatorial elections 1954 to 1964, average voter turnout in congressional elections 1962 and 1964; Democratic success: the Democratic percentage of the two-party vote for Governor 1954 to 1964, and the Democratic proportion of seats in the upper and lower chambers of state legislatures 1954 to 1964; malapportionment: the index of representatives suggested by Manning J. Dauer and Robert G. Kelsay "Unrepresentative States," *National Municipal Review,* 44 (December, 1955), 571–575, updated to 1960; the index of urban underrepresentation suggested by Paul T. David and Ralph Eisenberg, *Devaluation of the Urban and Suburban Vote* (Bureau of Public Administration, University of Virginia, 1961); and the apportionment score suggested by Glendon Schubert and Charles Press. "Measuring Malapportionment," this REVIEW, 58 (June, 1964), 302–327, with corrections published December, 1964, pp. 966–970; interest group strength: judgements of respondents reported by Belle Zeller, *American State Legislatures* (New York: Crowell, 1964), pp. 190–191; length of constitution, number of constitutional amendments

TABLE 4. RELATIONSHIPS BETWEEN INCOME INEQUALITY AND SELECTED POLICY VARIABLES IN THE AMERICAN STATES[11]

Selected Policy Variables	Coefficients Obtained with Gini Index	Compared to coefficients obtained with specific levels of:				factor scores for:	
		Income	Industrialization	Education	Urbanization	Industrialization	Cultural Enrichment
Education							
Per pupil Expenditures	−.61	.83*	.36	.59	.51	.45	.61
Educ. Exp. % of Income	.19	−.28	−.44*	.05	−.27	−.58	.12
State % Educ. Exp.	.45*	−.23	.18	−.35	.09	.09	−.39
Average Teach. Salaries	−.54	.88*	.64	.57	.69	.58	.53
Size School Districts	.36	−.18	.26	−.39*	.06	.04	−.43
Teacher-Pupil Ratio	.55	−.43	.19	−.50	−.12	−.05	−.56*
Drop-out Rate	−.62	.54	−.08	.60	.40	−.22	−.64*
Mental Failures	−.62	.46	.13	.70*	−.05	.20	−.66
Health & Welfare							
Unemployment Benefits	−.68	.80	.30	.67	.54	.39	.66
ADC Benefits	−.78*	.74	.26	.55	.61	.42	.56
General Asst. Benefits	−.68	.76*	.39	.43	.58	.55	.42
ADC Recipients	.41	−.30	.16	−.42	−.15	−.04	−.44*
Unemploy. Recipients	−.53	.58	.69*	.23	.39	.60	.23
Per Cap. Welfare Exp.	.04	−.01	.07	.07	.19	−.02	.12
Per Cap. Health Exp.	−.34	.56*	.39	.42	−.45	.27	.44
State % Welfare	.37	−.35	−.14	−.17	−.11	−.38*	−.12
State % Health	.09	−.08	−.07	−.14	−.30*	−.04	−.24
Taxation							
Total Revenue Per Cap.	−.50	.64	.03	.75*	.29	−.13	.75*
Per Cap. Tax Revenue	−.66	.76*	.23	.74	.59	.28	.77*
Taxes % of Income	−.07	−.17	−.51*	−.16	−.15	−.45	.16
State % Taxes	.49*	−.33	−.08	−.24	−.28	−.39	−.32
Sales Tax Reliance	.13	−.15	−.01	−.18	−.03	−.08	−.12
Income Tax Reliance	−.05	.20	.02	.19	−.04	.08	.05
Highways							
Per Cap. Highway Exp.	.26	.01	−.51*	.36	−.37	−.54	−.36
State % Highway Exp.	.06	−.15	.04	−.03	−.29	−.30*	−.01
Highway Fund Diversions	.05	.06	.29	−.05	.42*	.31	.02
Rural-Urban Distribution	.29	−.38	−.45*	−.07	−.35	−.42	−.16
Highway User Revenue	.19	−.32*	−.20	−.26	−.04	−.12	−.17

* Highest simple coefficient among independent variables.

Cross-sectionally speaking, an increase in income inequality is associated with a reduction in party competition for state office, a reduction in voter participation, an increase in Democratic voting for state offices, an increase in interest group strength, more fragmentation in state executive organization, and a reduction in the governor's formal powers. But what is even more noteworthy is the fact that the distribution of income is *more* closely related to these political system variables than absolute measures of social and economic resources in the states or factors derived from such absolute measures. The coefficients obtained with the Gini index are higher than those obtained with measures of the level of social and economic resources or with environmental factor scores.[10] These admittedly rough calculations suggest that the *distribution* of social and economic resources within a state may be more important politically than the *level* of social and economic resources.

A different picture emerges, however, when we examine the relationships between income in-

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and number of elected state agency heads: see Lewis A. Froman, "Some Effects of Interest Group Strength in State Politics," this REVIEW, 60 (December, 1966), 952–962; governors formal power index: see Joseph M. Schlesinger, "The Politics of the Executive" in Herbert Jacob and Kenneth Vines (eds.), *Politics in the American States* (Boston: Little, Brown, 1965), p. 229.

[10] Unfortunately, the degree of inter-relatedness between income inequality (Gini) and income levels as well as other socio-economic variables render partial correlations and beta values unreliable as a means of testing the independent effect of income inequality on political system variables or public policy. A simple comparison of the size of the correlation coefficients is probably the most efficient method of comparing income inequality with levels of socioeconomic resources.

[11] The variables in Table 4 were selected from over 100 policy measures for which similar results were obtained. The specific measures in Table 4

equality and measures of the *level* of public spending and service in education, health, welfare, taxation and highways. While it is true that the Gini index correlates with a large number of policy outcome measures in these fields (see Table 4) the coefficients obtained with the Gini index are usually not as high as those obtained with specific socio-economic indicators or with factors reflecting environmental dimensions. (The only policy variables which are more closely related to Gini than to levels of socio-economic resources deal with centralization in state-local relations, e.g., state percent on total educational expenditures, state percent of total welfare expenditures, and the state percentage of total tax revenues. This means that income inequalities are associated with centralization of governmental spending and services at the state level.) This suggests that overall *levels* of public service in the states are more closely related to *levels* of social and economic resources than to the *distribution* of these resources.

The purpose of this note is to suggest that the distribution of resources within the states may have a significant impact on the character political systems and the content of policy output. Levels of economic development are certainly influential determinants of politics and public policy, but it is also probable that the distribution of economic resources also shapes politics and public policy in the states.

This brief exercise with a single measure of the distribution of resources suggest an interesting hypotheses for further study: The *distribution* of resources within American states are more important in shaping state *politics* than *levels* of socio-economic development; but *levels* of socio-economic development are more influential in determining *levels of public spending and service* in the states than the *distribution* of resources.

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are: Education: per pupil expenditures in ADA 1961–62, educational expenditures as a percent of total personal income 1961, state percentage of total state-local expenditures for education 1961, average teachers salaries 1961–62, average size of school district 1961–62, teacher pupil ratio, high school graduates in 1963 as a percent of 9th graders in 1959, percent of selective service examinees disqualified for failing mental test 1962; Health and Welfare: unemployment compensation average weekly payment 1961, ADC average monthly payment 1961, general assistance average monthly payment 1961, ADC recipients per 10,000 population 1961, per capita state-local welfare expenditures 1961, per capita state-local health expenditures 1961, state percentage of total state-local expenditures for welfare, state percentage of total state-local expenditures for health; taxation: total state-local revenue per capita 1961, taxes as a percent of personal income 1961, state percentage of total state-local tax revenue 1961, sales taxes as a percent of state tax revenue 1961, income taxes as a percent of state tax revenue 1961; Highways: per capita state-local expenditures for highways 1961, state percentage of total state-local expenditures for highways 1961, percentage of highway revenues diverted to non-highway purposes 1961, percent of state highway grants going to counties rather than municipalities 1961, highway user revenues as a percent of total state revenue 1961.

THE CLASS BASIS OF ARGENTINE POLITICAL PARTIES*

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Students of Latin American political parties have long assumed a strong correlation between social class and party identification, yet this assumption has never been tested empirically in any of the Latin American nations. This is probably due in large part to the lack of reliable survey data; however, even the mass of aggregate voting data has seldom been analyzed systematically. As a result, most of what we know—or think we know—about voting behavior in Latin America is based upon the intuitive assumptions of North American scholars. "If I were a member of the Chilean middle class, I would probably vote for the Radicals or Christian Democrats, but on the other hand if I were quite wealthy I would probably vote for the Conservatives."

Students of Argentine politics assume that the Conservative parties, always representing the interests of the nation's aristocracy, have received the bulk of their votes from the large landowners; that the interests of this group consistently have been opposed by the Radical parties who receive their electoral support from the urban middle classes; and that the more recently formed Peronist parties have championed the cause of, and been supported at the polls by, the industrial workers. It is the purpose of this article to test these assumptions, primarily through analysis of aggregate voting data, but also by examining the social backgrounds of party leaders and their actions while in power.

I

Contrary to the popular myth, it now appears that prior to the 1943 revolution very few differences existed between the social backgrounds of the leaders of Argentina's major political parties, and also that correlations between social class and voting behavior were quite insignificant. Gallo and Sigal in their study of candidates for major elective offices in 1916 conclude that "as far as occupation, nationality and education are concerned, the Radicals do not differ in any way from their Conservative counterparts."[1] In each case the vast majority of the candidates came from the well-educated upper and upper-middle classes.[2] The same was true of the members of the national committees of these parties, almost all of whom were recruited from the economically privileged classes prior to 1943.[3]

TABLE 1. RANK ORDER CORRELATIONS BETWEEN OCCUPATION AND VOTING IN THE FEDERAL CAPITAL: 1940

Occupation	Conservatives	Radicals
All workers	−.076	−.243
Industrial workers	−.268	−.340
Public service workers	−.004	−.196
All employees	+.104	+.353
Public employees	+.238	+.250
Professionals	+.316	+.226
Industrialists	−.262	−.032
Businessmen	−.217	+.272

Source: Gino Germani, *La estructura social de la Argentina* (Buenos Aires: Editorial Raigal, 1955), pp. 253–255.

There is also some evidence which indicates that occupation had little effect upon voting behavior during this period. Using aggregate data, Germani found only very weak correlations between occupation and party vote in the 1940 congressional elections in the Federal Capital—

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* Much of the data included in this article was obtained between September 1966 and August 1967 while the author was in Argentina. This trip was made possible by the generous financial assistance of the Henry and Grace Doherty Foundation and the University of Iowa Research Council.

[1] Ezequiel Gallo and Silvia Sigal, "La formación de los partidos políticos contemporáneos: la UCR (1890–1916)," in Torcuato DiTella et al. *Argentina, Sociedad de Masas* (Buenos Aires: Editorial Universitaria de Buenos Aires, 1965), p. 163. A separate study shows the same to have been true of those who were elected to Congress in 1916. See Dario Canton, *El Parlamento Argentino en épocas de cambio* (Buenos Aires: Editorial del Instituto, 1965), pp. 52–66.

[2] According to Gallo and Sigal (*op. cit.*, pp. 163–164) 69% of the Conservatives and 71% of the Radicals came from these classes, while 87% of the Conservatives and 91% of the Radicals had a university education.

[3] See José Luis de Imaz, *Los que mandan* (Buenos Aires: Editorial Universitaria de Buenos Aires, 1964), p. 185.

and these correlations are even less significant when compared to those of 1946.

Huerta Palau comes to the same conclusion with regard to the 1935 gubernatorial election in Córdoba. There, in spite of the fact that the Radical candidate (a country doctor) waged a vigorous campaign designed to appeal to both urban and rural laborers, while the candidate of the Conservatives (himself a large landowner) obviously represented the interests of the traditional upper class, there was virtually no correlation between occupation and party vote.[4]

The popular view of the Radicals of this period as representatives of the urban middle classes who consistently opposed the interests of the landowning aristocracy is further refuted by the actions of Radical congressmen. For example, Peter Smith has shown that between 1916 and 1930, while the Radicals were in control of the national government, ninety bills which obviously were favorable to the interests of the livestock producers were presented in Congress; Radicals introduced sixty per cent of these bills, the Conservatives twenty-nine per cent, and Progressive Democrats and Socialists the remaining eleven per cent.[5]

II

The picture suggested by the above studies changed a great deal after the rise of Peronism, and it now appears that since 1943 there have been substantial differences in the social composition of Argentina's major political parties—at least between the Peronists on the one hand, and the non-Peronists on the other. As far as party leaders are concerned, this difference may be seen in the class structure of the various national committees. In the twenty-five year period between 1936 and 1961 over ninety per cent of the Radical and Conservative national committee members were from the upper or upper-middle classes, while the remainder were uniformly from the lower-middle class; in contrast, over a third of the Socialists came from the lower-middle class, and almost a third of the Peronists were lower class in origin.

TABLE 2. SOCIAL COMPOSITION OF NATIONAL COMMITTEES: 1936–1961

Class	Conservative	Radical	Socialist	Peronist
Upper and upper middle classes	97%	90%	62%	63%
Lower middle class	3%	10%	36%	5%
Lower class	—	—	2%	32%
N	39	53	52	79

Source: José Luis de Imaz, *Los que mandan* (Buenos Aires: Editorial Universitaria de Buenos Aires, 1964), p. 102.

Additional differences in party leadership may be seen in Table 3 which gives the primary occupation of Argentine congressmen serving in 1890 (when the Conservatives ruled unchallenged), in 1916 (the first year in which a large number of Radicals were elected), in 1946 (the first year of Peronist domination), and in 1966 (the last year the Congress met). In 1916 the differences in occupation between Conservative and Radical congressmen were relatively insignificant. Members of the liberal professions dominated both delegations; however, about one and a half times as many Radicals as Conservatives were landowners (just the opposite from what the popular myth would lead one to expect). In the 1946 Congress the differences between Radicals and Peronists were far greater. Over half the Radical congressmen were lawyers and 84% were members of one of the liberal professions; less than a third of the Peronists were lawyers, and only half of them were professionals of any type. More important, 17% of the Peronist congressmen were classified as white collar workers (*empleados*), and another 17% as laborers (*obreros*). None of the Radicals fell into these categories; in fact it seems quite likely that these Peronists were the first of their occupation ever to serve in Congress.

Basically the same differences were repeated in 1966 when three-fourths of the Radicals and almost all of the Conservatives were members of the liberal professions, while this was true of less than half of the Peronists. In contrast, 31% of the Peronist congressmen were either white collar workers or laborers versus 4% of the Radicals and none of the Conservatives. It might also be noted that the largest percentage of landowners was to be found not among the Conservatives, but among the Peronist delegation.

It also appears that occupation became an important factor in voting behavior after 1943. As was mentioned above, Germani has shown that in the 1946 congressional elections in the Federal Capital the correlations between occupation and party vote were extraordinarily high —and one should remember that just six years earlier such correlations virtually did not exist. The predominantly working class districts tended to vote Peronist, while Radical candidates fared best in districts in which there were large numbers of white collar workers, members of the liberal professions, and businessmen (the

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[4] Pedro Huerta Palau, *Análisis electoral de una ciudad en desarrollo* (Córdoba: Universidad Nacional de Córdoba, 1963), p. 27.

[5] Peter Smith, "Los radicales argentinos y la defensa de los intereses ganaderos, 1916–1930," *Desarrollo Económico* 7 (April–June, 1967), p. 826.

TABLE 3. PRIMARY OCCUPATION OF ARGENTINE CONGRESSMEN

Occupation	1890	1916		1946		1966		
	Con	Con	Rad	Rad	Per	Con	Rad	Per
Lawyers	65%	66%	57%	51%	32%	71%	36%	31%
Doctors	10%	10%	13%	18%	9%	18%	17%	4%
Other liberal professions	4%	7%	7%	15%	9%	—	21%	10%
Landowners	8%	15%	23%	10%	12%	6%	7%	10%
Teachers	4%	2%	—	3%	2%	—	1%	4%
Military	9%	—	—	3%	3%	—	—	—
White collar	—	—	—	—	17%	—	3%	23%
Laborers	—	—	—	—	17%	—	1%	8%
Other	—	—	—	—	—	6%	13%	10%
N	78	59	44	39	106	17	94	49

Source: Dario Canton, *El parlamento argentino en épocas de cambio* (Buenos Aires: Editorial del Instituto, 1966), pp. 40, 55–56; and data supplied the author by the División Información Parlamentaria de la Cámara de Diputados de la Nación.

last mentioned to a somewhat lesser extent). In contrast, there was no significant correlation between the number of industrialists in a district and the vote received by either party.

Huerta Palau's study of the 1962 election in Córdoba shows basically the same thing—although the correlations are lower. The Peronists received the bulk of their votes in districts in which there were large numbers of workers and/or farmers, while faring quite poorly in areas dominated by students, professionals and businessmen. Virtually the reverse was true of the two Radical parties, although the Intransigent Radicals fared much better than the People's Radicals in districts composed of large numbers of businessmen.

III

Some additional indication of the effects of social class upon Argentine voting behavior can be gained from an examination of election returns in each of the twenty districts into which the Federal Capital is divided. In each of the six post-Perón elections the wealthier northern districts cast more Conservative and fewer Peronist votes than did the poorer southern districts. However, these districts are far too large and heterogeneous to be of great use. Fortunately, the results of the 1957 election are available for each of the Federal Capital's 209 circuits (roughly equivalent to precincts in the United States). As far as social composition is concerned these smaller units are much more homogeneous than the districts, and thus more suitable for an analysis of the relationship between class and voting.

Fifteen circuits were selected for analysis. Four of them would be considered upper class by almost any standard; two are in the Retiro

TABLE 4. RANK ORDER CORRELATIONS BETWEEN OCCUPATION AND VOTING IN THE FEDERAL CAPITAL: 1946

Occupation	Peronists	Radicals
All workers	+.973	−.908
Industrial workers	+.898	−.966
Public service workers	+.920	−.802
All employees	−.653	+.692
Public employees	−.698	+.720
Professionals	−.869	+.918
Industrialists	−.095	−.036
Businessmen	−.515	+.435

Source: Gino Germani, *La estructura social de la Argentina* (Buenos Aires: Editorial Raigal, 1955), pp. 253–255.

TABLE 5. PRODUCT MOMENT CORRELATIONS BETWEEN OCCUPATION AND VOTING IN THE CITY OF CORDOBA: 1962

Occupation	Conservative	Peronist	UCRP	UCRI
Farm laborers	−.558	+.587	−.544	−.471
Skilled labor	−.541	+.631	−.791	−.931
Farmers	−.454	+.324	−.148	−.304
Employees	+.020	−.052	+.024	−.006
Students	+.908	−.895	+.489	+.714
Professionals	+.406	−.866	+.519	+.742
Businessmen	+.330	−.999	−.295	+.267
% of the total vote	4%	36%	28%	14%

Source: Pedro Huerta Palau, *Análisis electoral de una ciudad en desarrollo* (Córdoba: Universidad Nacional de Córdoba, 1963), p. 19.

TABLE 6. VOTING BEHAVIOR IN SELECTED CIRCUITS IN THE FEDERAL CAPITAL: 1957
(in percentages)

Circuit type	Com	Soc	Peron	UCRI	UCRP	PDC	Con	PCI	other
Upper class	2.0	7.2	9.2	13.9	26.1	13.4	6.7	12.1	8.6
Upper-middle	2.4	11.2	13.2	15.8	28.5	8.7	2.8	8.7	8.9
Lower-middle	4.0	16.2	21.3	19.4	25.2	4.4	0.9	1.1	7.5
Lower class	6.2	10.6	28.1	20.7	21.8	2.8	0.9	0.8	6.2
Entire capital	4.1	14.7	18.4	19.5	26.3	5.7	1.7	2.7	6.9
All Argentina	2.6	6.0	24.3	21.2	24.2	4.8	6.5	1.0	9.4

Note: The upper class circuits are numbers 194, 200, 203 and 204; the upper-middle class circuits are 163, 166 and 168; the lower-middle class circuits are 32, 33, 174 and 175; the lower class circuits are 7, 8, 10 and 13.

area, which according to Imaz[6] has almost half of the Buenos Aires aristocracy, while the other two are in Barrio Parque, the one part of the city composed exclusively of large expensive houses. The three circuits labeled upper-middle class are in Barrio Belgrano near the *barrancas;* this is an area composed primarily of large, new, expensive apartment houses. Two of the lower-middle class circuits are located in Colegiales and the other two in La Boca; in each area older and cheaper apartment houses are predominant. These two widely separated neighborhoods give the impression of being "run down" but not poverty stricken. The four lower class circuits are in the slaughter house and meat packing *barrios* of Nueva Chicago, Villa Lugano and Nueva Pompeya.

One additional point pertaining to the social composition of these circuits must be mentioned. This is the fact that almost all upper and upper-middle class families in Buenos Aires employ "live-in" domestic servants who vote in the same circuits as their employers. While many lower-middle class families also have servants, they are much less likely to live-in, and thus do not affect this analysis to any appreciable extent. It is thus only the lower class circuits that are completely homogeneous in character.

As can be seen in Table 6, the percentage of the vote received by the two conservative parties (the business-oriented Independent Civic Party and the more rural-based Conservative Party) declines as one goes down the social scale. Virtually the same is true of the Christian Democrat vote, except that while the conservatives received almost as few votes from members of the lower-middle class as from the lower class, the former was somewhat more inclined to vote for Christian Democrats. This picture was reversed in the case of the Communists and Peronists and, to a limited extent, the Intransigent Radicals. The first two parties received more than three times as many votes in lower class circuits as in upper class ones, while the UCRI vote increased gradually from 13.9% to 15.8% to 19.4% and finally to 20.7%.

The People's Radicals fared slightly better among upper-middle class voters (28.5%) than among upper class and lower-middle class (26.1% and 25.2% respectively), while their worst showing was a still quite formidable 21.8% among the lower class. Virtually the reverse was true of the Socialists who received somewhat more votes from the lower-middle class than from either the upper-middle or lower classes (16.2% versus 11.2% and 10.6%); their poorest showing was in the upper class circuits where they received only 7.2% of the vote.

If one assumes that the Independent Civic and Conservative parties are rightist, the two Radical parties and the Christian Democrats centrist, and the Communists, Socialists and Peronists leftist—as both their programs and actions would indicate—it becomes apparent that the vote for center parties is remarkably stable across class boundaries, with only the extremist vote shifting appreciably with social class.

It is most important to note that while the conservative parties derive virtually all their electoral support from the upper and upper-middle classes, Peronist support is by no means limited to the lower class, nor are the Radical voters uniformly from the middle classes. While most of the Peronist votes cast in the upper and upper-middle class circuits may well have come from domestic servants, this is almost certainly not the case in the lower-middle class circuits where over 20% of the voters supported Peronist candidates. It should also be noted that the supposedly middle class Radicals received far more votes in the lower class circuits than did the Peronists (42.5% versus 28.1%). Likewise, the two Radical parties garnered twice as many votes as the Conservative parties in the upper class circuits.

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[6] José Luis de Imaz, *La clase alta de Buenos Aires* (Buenos Aires: Universidad Nacional de Buenos Aires, 1965), p. 15.

Thus, judging from the electoral behavior of the inhabitants of these fifteen circuits, it seems safe to conclude that Argentine conservatism is limited in appeal to the upper and upper-middle classes, and that Peronism is largely a movement of the lower and lower-middle classes; however, it is obviously inappropriate to turn this around and claim that the upper class is predominantly conservative in its voting habits, or that "the Peronists speak for a majority of the wage earners."[7] Also, the Radicals, who received between forty and forty-five per cent of the total vote in each of the four categories of circuits, must be considered multi-class rather than merely representative of the middle sectors of society.

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[7] Robert J. Alexander, "The Emergence of Modern Political Parties in Latin America," in Joseph Maier and Richard W. Weatherhead (eds.), *The Politics of Change in Latin America* (New York: Frederick A. Praeger, 1964), p. 107.

PROCEDURES FOR REVIEWING MANUSCRIPTS

AUSTIN RANNEY, MANAGING EDITOR

In response to several inquiries and a suggestion from the Editorial Board, I present here a brief description of the REVIEW's procedures for reviewing manuscripts. The reader should be clear that what follows is *not* a discussion of substantive editorial standards comparable to that published by my predecessor (Harvey C. Mansfield, "Toward a Definition of Editorial Policy for the REVIEW," 56 [March, 1962], 129–138); it is simply an account of the procedures we follow in evaluating and deciding on the manuscripts submitted to us.

Objectives. Our reviewing procedures are intended to accomplish four main objectives: First, to select from among the manuscripts submitted those most suitable for publication. Second, to work the manuscripts we accept into the best possible shape before they appear in the awful finality of print. Third, to give every author who submits a manuscript a serious professional critique of his work whether we publish it or not. And fourth, to get a decision to the author within a reasonably short period of time (we try for, but do not always achieve, a maximum of two months).

Preliminary Screening. After each new manuscript is received, acknowledged, and logged, I leaf through it to determine whether it meets at least a minimum standard of scholarly quality. I estimate that about 95 per cent of all manuscripts pass this not-very-demanding test (not nearly demanding enough according to some of my reviewers), but a few do not. An example of the latter is a mimeographed speech on the Perils of Big Government made to a convention of commercial advertisers by the president of an advertising firm and submitted to the REVIEW by one of the firm's account executives.

Professional Review. The manuscript is then sent out for review, usually to at least two referees but sometimes to one. Each referee is chosen by me, often on the recommendation of a member of the Editorial Board, because he is regarded as an expert in the content area and/or special methodology covered by the manuscript and also as a good bet to answer his mail and meet a deadline. In the past three years I have used well over two hundred different persons as referees for one or more—usually more—manuscripts each.

We try, not always successfully, to ensure maximum objectivity in the reviews by preserving the author's anonymity; hence, his name and identifying footnotes and other references are removed before the manuscript is sent out. The reviewer is asked in a covering letter to comment on the manuscript's strong and weak points and to give us his net judgment as to whether the article is (1) publishable as it stands, (2) potentially publishable if certain changes, which the reviewer is asked to spell out, are successfully made, or (3) not publishable with any foreseeable changes. The reviewer is also given a deadline for delivering his comments to us, generally set at a month to six weeks after his receipt of the manuscript.

Most referees meet these deadlines faithfully and deliver thoughtful and often extended comments. I repeat here my conviction, expressed in each of the last three Annual Reports, that the reviewers' prompt, skilled, and conscientious performance of these demanding but unpaid professional chores is by far the most important single contribution made to whatever approach we make to achieving the objectives set forth above. A few referees, for one reason or another, do not meet their deadlines. They are often rewarded with a telephone call from the Managing Editor, which usually, though not always, stimulates delivery of their comments.

If the referees of a particular manuscript report substantially different evaluations and/or if I remain in doubt about what to do with it, I send it to one or two additional referees. The final decision, of course, is the Managing Editor's alone. But the content specialties in contemporary political science are so multifarious and the methodological problems and techniques so diverse that I—and, I like to think, any Managing Editor—must rely heavily on expert advice for evaluating manuscripts in most fields other than my own.

Decision. When all the reviews are in, I read them carefully, compare and synthesize their evaluations, often look over the manuscript again myself in the light of the reviews, and eventually come to one or another of three decisions:

(1) To accept for publication. In this case I write to the author informing him that his article is accepted and indicating the issue in which it is scheduled to appear. I also stipulate a deadline for receiving the final version of the manuscript to be copy-edited and prepared for the printer (the deadline at present is approximately three-and-one-half months prior to the

month of publication). I enclose copies of the readers' comments* and make whatever suggestions for changes seem desirable in preparing the final version.

(2) To reject. In this case I write to the author informing him that we cannot publish his manuscript and stating the reasons for the decision. Copies of the readers' comments, which usually set forth these reasons in more detail, are also enclosed so that the author has at least gained a serious professional critique of his work.

(3) To suggest revision and re-submission. In this case I inform the author that we cannot publish his manuscript in its present form, state the reasons for the decision, and enclose copies of the readers' comments. I then invite him to re-analyze his data and/or re-write his presentation along certain specified lines and to submit the revised version for further consideration. It is made clear that this invitation cannot be a guarantee to publish the revised version but is, rather, an expression of interest in considering it. I also acknowledge that the author may feel his article is publishable as it stands and may wish to submit it elsewhere, and I ask him to tell us what he intends to do. If he then prepares and submits a revision, as many in this category do, the new version is put through the reviewing process again, usually with one of the readers of the first version serving as a referee of the second.

The Results. From August 1, 1965 to July 31, 1968—the period covered by the first three Annual Reports of my tenure as Managing Editor—a total of 727 manuscripts were submitted. Of these, 122 (16.8 per cent) were accepted for publication in either their original or revised versions; 60 (8.2 per cent) were returned with an invitation to revise and re-submit, but as yet no revised versions have been received; and 545 (75.0 per cent) were rejected.

I mentioned at the outset that this note would not deal with the substantive standards applied by the journal's advisers, referees, and Managing Editor in evaluating and making decisions on manuscripts—a much larger and more complex topic than any discussed here. I hope, however, that this brief account of our decision-making machinery will be of interest to all our readers and useful to those who contemplate submitting manuscripts.

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* We also try to ensure maximum objectivity and frankness in our reviews by making every effort to conceal the referees' identities from the authors. Hence we remove all identifying marks and phrases from the reviews before sending them on.

COMMUNICATIONS

To the Editor:

I have read with some astonishment in the appendage to the lead communication in the December Review that "the Managing Editor regrets that inadequate copy-editing and proofreading allowed this unwarranted statement to appear in the Review and apologizes to Professor Spiro and the Dorsey Press. . . ." I want to make it clear that I do not doubt the good intentions of the Managing Editor. However, in view of the fact that I refused to retract in that kind of form and of the possible, and I believe unintended, inferences that can be drawn from the wording of the retraction, I consider it essential for the good of the profession and the reputation of the Review that the Managing Editor affirm the following three points in the next issue of the Review. 1. He will not change a review substantively without permission of the writer. 2. He will permit criticism of publishers in reviews. 3. If he had asked me to revise my review, he would not have asked me to retract my criticism of the Dorsey Press but instead would have asked me to revise it in less colloquial language.

One issue—and I believe it will disappear when the Managing Editor replies—is whether pressure from a publisher will produce retractions or deletions before publication. A second involves the responsibility of a publisher of an elementary undergraduate textbook to the thousands of young students who might use it and to the many teachers, who, as is true of all of us, cannot be equally competent in all aspects of the subject matter, for the accuracy of the textbook in the presentation of facts and in the representation of the intellectual positions of other scholars. Accurate representation is essential if the introductory textbook, as is true in this case, undertakes a polemical criticism of other points of view. I concede the author's contention that he received more advice than he used. However, the book did appear under the Dorsey imprimatur and they retain a responsibility regardless of whether they did not engage enough readers, chose their readers unwisely, or merely failed to insist that the author use the good advice that was offered to him. The publisher need not be held responsible for advanced or specialized books that are intended for professionals or for use in class by specialists. Subject matter introductions entail a definite degree of responsibility on the part of the publisher. It is essential in view of this that reviewers be free to call attention to this issue.

Morton A. Kaplan

University of Chicago

(Editor's Note: In response to Professor Kaplan's first two points, I am happy to reaffirm longstanding policies of the Review: (1) Neither the Book Review Editor nor I have changed or will change a review substantively without permission of the reviewer—as, for example, we did not in Professor Kaplan's review of Professor Spiro's book; (2) both the Book Review Editor and I have permitted and will continue to permit criticism of publishers in reviews—as we did in Professor Kaplan's review; and (3) no pressure from a publisher will produce retractions or deletions before publication—as they did not in Professor Kaplan's review.

His third point, and indeed this whole controversy, turns on one's interpretation of the following statement in his review: "Professor Spiro is not to be blamed for [the inadequacy of the discussion of strategic doctrine], but it is a comment on the Dorsey Press that they *could not afford* proper critical help to the author for a book being offered as an undergraduate text" (issue of June, 1968, p. 694, italics added). The Dorsey Press contended that this statement would be construed to mean that their financial condition did not enable them to pay for proper critical help for Professor Spiro. Professor Kaplan contended, if I understand him rightly, that the phrase "could not afford" was a colloquial usage meaning "did not provide" and was not intended as a literal comment on the Dorsey Press's financial condition.

Had the Book Review Editor (Professor James W. Prothro at that time) or I caught this phrase in our copy-editing or proof-reading, we would have suggested to Professor Kaplan a wording not subject to the construction the Dorsey Press placed on it; but we would *not* have suggested any alteration in Professor Kaplan's substantive judgment that the content of the section concerned seemed evidence of inadequate prepublication review and advice. That, it seems to me, is fair comment for a scholarly reviewer; but a phrasing which suggests that the inadequate advice stemmed from inadequate financial resources is not. However, neither of us caught the phrase, and it was published as Professor Kaplan wrote it.

That being the case, on legal advice I made the apology published in Communications in the December, 1968 issue (p. 1268). I also informed both the Dorsey Press and Professor Kaplan that we would publish any statement by Professor Kaplan acceptable to both him and the Press. Professor Kaplan prepared a statement which was rejected by the Press as inadequate, and so I did not publish it. I then informed the Press that (1) neither I nor the American Political Science Association could or would try to persuade Professor Kaplan to alter his statement; (2) my statement discharged the REVIEW's obligation; and (3) the REVIEW and the Association would not be involved in any subsequent negotiations between the Dorsey Press and Professor Kaplan. That continues to be my position.)

TO THE EDITOR:

What with all the sophisticated, divergent, and often highly abstract approaches to international politics appearing in this journal and elsewhere, this correspondent wishes to report a scheme which is as simple and conclusive as is its promise for general acceptance by the profession. Entitled—tentatively, of course—"The Summer Olympics of 1968: The Index of RP&I in the International System," it goes like this:

I. Problem and Solution: Students of international politics have long sought quantitative indices to determine a nation-state's RP&I ("relative power and influence") in the international system. Several indices have been suggested so far, including military force, GNP, size of population, level of economic development, economic aid, length of foreign broadcasts, and the number of diplomats received or sent abroad. Some have attempted to combine these indices in multivariate analyses. That such indicators or indices—separately or collectively—are unsatisfactory becomes obvious, however, when one postulates that only athletic events in general and the Olympic Games in particular generate world-wide attention, to wit, they are universally conceived as the only reliable indication of power. Accordingly, the systematic analysis of the results of olympic competition will provide a verifiable empirical referent to the measurement of power at the same time as it will narrow our understanding of the subject at hand.

II. Power Performance Quotient (PPQ), or a Word about Methodology: The frequently employed index of medal standing by nation-states was considered unrevealing and unsophisticated. Instead, this correspondent's own method of PPQ ("power performance quotient") was used: it relates NMW ("number of medals won") to the nation-state's population. PPQ, therefore, answers the question: how many people did it take to win a medal? It goes without saying that the smaller the PPQ, the larger the RP&I.

III. Revealing Results:

Ranking	*Nation-State*	*NMW*	*Population** (in millions)	*PPQ* (in) thousands)
1	Mongolia	4	.9	230
2	Hungary	32	10	310
3	Denmark	8	5	620
4	Australia	17	11	640
5	E. Germany	25	17	680
6	Kenya	9	7	780
7	Bulgaria	9	8	890
8	Switzerland	5	5	1,000
9	Czechoslovakia	13	14	1,070
10	Finland	4	7	1,220
11	Rumania	15	19	1,260
12	Netherlands	7	11	1,570
13	U.S.	107	179	1,670
14	Poland	18	31	1,720
15	Austria	4	7	1,750
16	Cuba	4	7	1,750
17	Sweden	4	8	2,000
18	W. Germany	25	58	2,320
19	USSR	91	214	2,350
20	Yugoslavia	8	19	2,370
21	Italy	16	50	3,120
22	France	14	47	3,350
23	Canada	5	18	3,600
24	Japan	25	93	3,720
25	Mexico	9	35	3,900
26	Gr. Britain	13	53	4,070

* Based on census figures.

IV. Conclusions and Suggestions for Further Research: The systematic exploitation of our data must be left to others similarly committed to the scientific study of the allocation of power in international politics. Only one important conclusion—the urgent policy implication—will be offered here; to wit, since our data irrefutably point to the supremacy of Mongolia in the international system (with the Mongolian PPQ being ten times that of the USSR), those who have treacherously and irresponsibly promoted the idea that the USSR is the most potent challenger to U.S. power must, from now on, concentrate on the real threat: Mongolian power. In short, it is the imperative task of the new Administration to establish "clear-cut American superiority" over the Mongolian People's Republic.[1]

As to further research, it is to be hoped that it will be pursued rigorously and systematically to determine once and for all the balance of power in world politics on both the systemic and sub-systemic levels, including the distribution of power between East and West, North and South, and perhaps even between Northeast and Southwest and Southeast and Northwest. Moreover, studies of past Olympic Games could lead researchers to undertake trend analyses. Finally, our modest exploration also indicates the necessity of establishing a data bank on, and an institute for the study of, RP&I and PPQ in world politics. Such an institute could then generate further studies on the problems and symptoms of BS.

Union College and The
University of Kansas
(visiting)

CHARLES GATI

To THE EDITOR:

Permit me to respond in the REVIEW to the important gap between findings and interpretation offered by Langton and Jennings in the September, 1968 issue. Ken Langton and Kent Jennings are two of the most accomplished students of political socialization, and their large-scale Michigan study ought to have a major impact upon the profession. Therefore, "Political Socialization and the High School Civics Curriculum" raises several questions that are of interest to us all:

1. Why call for "a radical restructuring of these [civics] courses in order for them to have any appreciable pay-off" when it has been concluded that "our findings certainly do not support the thinking of those who look to the civics curriculum in American high schools as even a minor source of political socialization"?

2. Why conclude that the civics curriculum has so little impact, when the reported data show that courses in American Government and Democracy are important sources of knowledge, political efficacy, and civic role for Negro students? After all, haven't the major public efforts in curriculum and educational development been directed toward the black community and its school system?

3. Why consider the policy and value dimensions of "political socialization" (and the curriculum) when the operational definitions are so narrow (taking 0 versus 1 or more civics courses), there are no behavioral dimensions of political learning and participation (I wonder what those seemingly compliant Blacks were doing between classes in the Afro-American Student Association) and the concept and method of political socialization itself smuggles in numerous assumptions about political impact? I would now give one cheer for the impact of Problems in American Democracy on Black students *because* of Langton and Jennings' findings and *despite* their assumptions and methods.

4. Why reinforce conclusions about curriculum irrelevance by noting that selection for higher education may be confounded with socialization in the schools? Of course it may, but the more important point is that socialization (and the curriculum) influences who is selected. Changing the quality and rewards of school socialization are reflected in college admission practices. Enhanced personal and political efficacy among Blacks is likely to increase the number of Black students selected by the University of Michigan.

5. Why stress "information redundancy" rather than politically relevant uses of acquired attitudes and information in accounting for reported racial differences?

I raise these questions in a spirit of concerned colleagueship because the work of Langton and Jennings is of such high quality in professional and behavioral terms. But the terms of Political Socialization concept and method screen out significant public questions about education's impact on political learning and behavior. Robert Hess, David Sears, and I speak to this point independently in the Summer, 1968 *Harvard Educational Review* where Hess raises the issue most pointedly: "Political socialization is a concept which is difficult to apply to the process of political learning during periods of rapid social change or open conflict between major segments of a society. It is of little usefulness in the United States today." Beyond some constructive criticism about a single article the point ought to be taken seriously in deciding what questions are most important in research for the professional knowledge base of the current generation.

The University of Connecticut EDGAR LITT

To THE EDITOR:

We appreciate the opportunity to reply to the points raised by Professor Litt.

1. The logic of his first point appears to be based on a misreading of our article. For the great majority (90%) of high school students there is no evidence that the civics curriculum has a significant effect. We *did not* conclude,

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[1] In this context some significance must be attached to the report that, during a planned tour of Asia, Senator Edward M. Kennedy intends to stop in Ulan Bator.

however, that civics courses could not have an impact, only that it would take ". . . radical restructuring . . . for them to have an appreciable pay-off."

2–3. The importance of civics for the Negro subpopulation was emphasized in our analysis. We do not share, however, the reasoning behind Professor Litt's "cheer" regarding the impact of these courses. Negative relationships between politicization and course exposure among high status Negroes leaves little room for cheer. Their increasingly passive image of their citizenship role is particularly distressing. It should also be emphasized that the vigor of Afro-American movements had not yet penetrated this generation of Black students when the data were gathered (1965). Nor should we delude ourselves concerning the extent to which Negro high school students are even now intensely involved in these movements. Finally, it is not obvious that the major thrust of curriculum reform is directed toward Blacks; such was definitely not true around 1965.

4. Our data do suggest that differences in political orientations between those with high school versus college education—differences usually ascribed to the "education process" at the college level—could be laid to selection rather than collegiate socialization. This does not mean that prior socialization is unimportant in determining who enters college. Increased political efficacy associated with taking civics courses *might* raise the chances of lower status Negroes going to college. But there are two important qualifications. First, civics has no measureable impact on the efficacy level of either high status Negroes or Whites of any status. Second, most studies reveal that familial factors are the prime predictors of who enters college.

5. We do not understand completely the thrust of Professor Litt's fifth point. Information redundancy was stressed because it seemed useful in explaining our findings.

6. As for what comes under the rubric of political socialization, we apparently prefer a broader construction of the term than do some scholars. To suggest that the process stops with the advent of the first urban disorder or student protest strains our sense of what constitutes political socialization.

Kenneth P. Langton
M. Kent Jennings
University of Michigan

To the Editor:

Mickiewicz's review of my book, *Soviet Institutions, the Individual and Society* (Boston: Christopher Publishing Co., 1967, co-authored with Irene M. Hulicka) conveys more information about the calibre of the reviewer's scholarship than about the contents of the book. Whereas one might hope that the misinterpretations on which she bases her review reflect only ignorance, there is room for suspicions about lack of professional ethics. A goodly portion of her discontent with our book is based on our "failure" to fulfill claims invented by her. As nearly as I can tell, the claims which she attributes to us are derived from her inaccurate interpretation of advertising material prepared by the publisher. It is, of course, possible that Mickiewicz is ignorant of the fact that authors are not responsible for the advertising statements made by publishers, but it would be unfair to her teachers to assume that she had never been taught that quotations which are attributed to others must be accurate both with respect to source and contents.

Since APSR policy allows a reviewer several thousands words to comment on a book, but allows the author only 400 words to reply to the comments, space limitations prevent me from discussing in detail Mickiewicz's misinterpretations, her amazing lack of differentiation between analysis and speculation, and her naïveté about institutions, events and processes in the Soviet Union. Moreover, her comments suggest that she failed to read the book; from the entire 680 pages she apparently scanned the 103 page chapter on the Communist Party of the Soviet Union, misread the publisher's statements on the cover pages and the authors' statements in the preface and introduction, and read the occasional sentence here and there elsewhere in the book. Had her comments been published in any journal other than the A.P.S. Review, I would not have even bothered to answer them.

Karel Hulicka
State University of New York at Buffalo

To the Editor:

Professor Hulicka's intemperate charges are unsupported and essentially unimportant. The major question, it seems to me, centers on the role of the reviewer, or for that matter, of the political scientist. Should an analyst of political affairs describe or reproduce official statements about a political system, or should he impose on that material categories of analysis derived from a theoretical source explaining or purporting to explain a wide variety of data? The official political doctrine of the Soviet Union does claim to provide the theoretical framework for meaningful categories of social and political analysis, but it is open to question whether Soviet

Marxism represents a systematic epistemology or simply a series of ad hoc explanations. In any case, the Western social scientist, unlike his counterpart in the Soviet Union, can openly look to more fruitful, more modern, more comparative theoretical sources. The chief failing of the Hulickas' book is that they have refused to go beyond the rather tautological theory of Soviet Marxism. Professor Hulicka is incorrect when he states above that I have invented claims which must be fulfilled by his book; I have imposed categories of social science analysis, it is true, but they are neither personally invented nor personally directed. They simply reflect the role which Western social science has set for itself. As I state in the conclusion to my review, the Hulickas' book "does present an unusually large collection of official Soviet statements and directives, but in comparison with the many excellent works currently available, it cannot stand as an adequate analytic study of the Soviet political system."

ELLEN MICKIEWICZ
Michigan State University

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BOOK REVIEWS

The Politics of School Desegregation. By Robert L. Crain. (Chicago: Aldine Publishing Company, 1968. Pp. 390. $7.95.)

This insightful analysis of school desegregation in fifteen American cities makes an important contribution to the urban politics literature. The skillful use of statistical methods (primarily rank order correlations) clearly demonstrates the feasibility of combining methodological rigor and sophistication with an interest in the most relevant political issues. The number of cases was small (usually N=7 or 8) because Crain wisely focused on systemic rather than individual attributes in explaining the inter-city variation in acquiescence to desegregation demands. For example, an ideological variable, the civil rights liberalism of the northern school boards, was directly and closely associated with acquiescence. Yet such liberalism was greatest in those cities where the largely reformist, good government—and civil rights oriented—social and civic elite had not moved into the suburbs. In fact, civil rights liberalism was more closely associated with political reformism than with conventional pro-New Deal economic liberalism—suggesting a major role for political theory in explicating the shift in ideological alignments that are now re-shaping the politics of American race relations. Acquiescence was also greater, however, in those northern cities where the low economic status of the entire population, and often a machine tradition, meant limited participation in school politics by anti-integrationist middle income whites.

In the South, the decisive factor was the elites' attitude rather than their power. Like many other southern, but few northern cities, New Orleans had an unmistakable, economically based political elite. But this elite's lack of sympathy for desegregation, and its lack of interest in school politics, permitted a violent, chaotic crisis to develop.

In some ways, school desegregation politics conforms to the pluralist model of interest group conflict, bargaining, and compromise over specific policies. Yet Crain's evidence at least implicitly suggests the presence of a fundamental, hardly pluralist cleavage which is more likely to emerge under some specifiable conditions than others. For example, concessions by the San Francisco school board only encouraged further civil rights militancy. We can hypothesize that, given the city's unusually participant political culture, such gestures encouraged expression of the fundamental but previously often latent interests of black Americans in eliminating pervasive racial inequality. Again, Crain found that across the entire country, the groups concerned with the symbolic or status goals of integration were less militant than some groups articulating the black community's material or welfare goals. This reversal of earlier findings may well reflect recognition by black Americans of their collective but previously latent interest in fundamental social change.

A similar latent interest analysis can be applied to important political structures. Crain and his colleagues suggest socio-psychological variables (rural small town backgrounds and initially low status) in explaining many school superintendents' civil rights conservatism. But desegregation demands may also have manifested the basic professional and bureaucratic interests of school administrations in resisting laymen's efforts to impose social change. Crain and his colleagues similarly suggest that "the professional (often machine) politicians . . . reject politics," (page 177), by resisting desegregation demands, because these politicians share the anti-Negro attitudes of lower status, poorly educated whites. Yet this conservatism may also reflect a patronage organization's interest in avoiding conflicts over fundamental social change lest it upset established patterns of pluralist bargaining and group accommodation. Finally, the relatively acquiescent attitude of many business elites may reflect a basic interest —made manifest both by the threat of civil conflict and by commitment to an entreprenurial ideology favoring economic growth—in preserving a peaceful business climate through concessions on desegregation.

The most problematic aspect of this generally excellent study is its method of data collection, since Crain sent two or three interviewers to each city, usually for one week. Their findings sometimes left important substantive questions unresolved (see page 80 on Baltimore's political culture) and produced few strictly quantitative indicators. Crain and his colleagues therefore tried to estimate the underlying variables directly, and thus avoid the unknown discrepancy between such variables and their indicators. But just because these direct rankings also lacked intensive *detailed* field work, the ambiguities which emerge were only resolved through research team discussions. These collective decisions were thus subject not only to the groups' shared biases as Crain recognizes, but less obvious distortions inherent in group dynamics—which may explain the otherwise inexplicable equating of Baltimore and Pittsburg when the authors' own criteria made Baltimore slightly more acquiescent (page 145).

The book is also somewhat perplexingly organized. The civil rights movement, which did not affect the policy outcomes, is considered entirely separately and might better have been written up only as a journal article, permitting expansion of the insightful, but overly dense and confusing chapter summarizing the northern cities. Two political variables, civil rights liberalism and election versus appointment to northern school boards, together account perfectly for inter-city variation in acquiescence. In turn, these two variables were associated with such other political factors as board cohesion, reform orientation, conflict tolerance and party strength. Yet Crain and his associates finally assert that the very high multiple correlation (r=.89) between acquiescence and low suburbanization and low socio-economic status eliminated most of the independent, as opposed to merely intervening, causal importance of all these political variables. But the evidence also indicates that the high correlation is, in part, spurious. In particular, we can explain St. Louis' highly acquiescent response not only because low SES offsets the high suburbanization of its elite, but because of the atypically pro-reform sentiments of its many relatively low status Germans. Again, suburbanization and socio-economic status evidently operate through several political variables in widely varying ways from city to city. Although SES and suburbanization predict Newark's moderate acquiescence correctly, this outcome was actually the result of the city's powerful political machine whose influence obliterated the effect of all other political as well as socio-economic factors.

These difficulties can be avoided by introducing new explanatory variables, which are constructed by the analyst in order to designate behavioral processes, interactions or in some cases "functions." For example, Crain's analysis suggests as such explanatory analytic variables the northern school boards' collective ideology and its independence from anti-integrationist pressures. The "concrete," largely static or structural attributes in Crain's analysis such as suburbanization, socio-economic status and machine strength and percent German in fact contributed to the boards' ideology and independence, but in different amounts in different cities. Introducing such analytic variables might well have eliminated both the unexplained variance and partly spurious relationships among their northern cities. This step would have also specified more precisely the casual role of the political variables which Crain had "left over" when suburbanization and SES were introduced.

The same procedure would also have permitted a single analysis of northern and southern cities. Of course, desegregation in the north, where the board was crucial, did not involve court litigation as in the south where the civic elite was decisive. Yet we would like to know, at least, if acquiescence within each region by itself had similar correlates, so that the most acquiescent cities in each region differed in the same ways, on the same explanatory dimensions, from the least acquiescent cities. To be sure, the most acquiescent cities in the North were low in status while the most acquiescent cities in the South were high. Nevertheless, as Crain and his colleagues suggestively observe, a pro-civil rights predisposition by the civic-economic elite was fairly wide-spread in the North, but its resources for influencing the school board in pluralistic local politics varied markedly with low suburbanization and low socio-economic status. In the South, the socio-economic elite usually had the necessary resources, but was likely to use them on behalf of desegregation only if—primarily in high status cities—it had appropriate traditions and values (page 360). In terms of analytically specified processes, the northern school boards and southern civic-economic elites were thus *behaviorally equivalent actors,* just as high status in the north and low status in the south were *behaviorally equivalent variables.* Acquiescence, therefore, depended in both regions on the independent position and pro-desegregation attitudes of the relevant elite. The low status of the entire population in northern cities, and high status in southern cities, were both correlated with acquiescence, because each of them was associated with the strength of the relevant elite and its pro-desegregation attitudes.

This reformulated proposition is neither tautological nor obvious. Logically, the concept of school desegregation does not specify an overwhelmingly decisive role for local elite attitudes and influence. Empirically, these two factors are much less important on such similar local issues as the War on Poverty or trade union influence. Of course, this proposition on elite attitudes and influence can only be tentative. The reliable specification of analytic processes requires that we identify those variables which have the same causal impact—and thus mean the same thing—to actors in different contexts. Even for scholars trained in the comparative analysis of urban politics, specification requires more information than that provided by three men doing a week's research. Since *The Politics of School Desegregation* is distinguished through out by admirable care as well as important insights, the author's failure to employ the analytic variables implicitly suggested by their own findings may well reflect a scholarly respect for the limitations of their data.

DAVID GREENSTONE

University of Chicago

Political Development in Modern Japan. EDITED BY ROBERT E. WARD. (Princeton: Princeton University Press, 1967. Pp. 637. $12.50.)

This large book is the fourth in a series of six large books dealing with the modernization of Japan. The series grows out of a special project of the Association of Asian Studies, financed by the Ford Foundation and the Carnegie Corporation, and is published by the enterprising Princeton University Press. Earlier volumes have dealt with concepts and theories of Japanese modernization (*Changing Japanese Attitudes Toward Modernization,* Marius B. Jansen, ed.), Japan's economic development and its political context and ramifications (*The State and Economic Enterprise in Japan,* William W. Lockwood, ed.), and with *Aspects of Social Change in Modern Japan* (R. P. Dore, ed.). Two more volumes will deal with cultural change and with dilemmas of growth in prewar Japan. Thus, like the "modern" society which is being analyzed, the present volume is a specialized and differentiated function of a well-financed bureaucratic structure, and must be appreciated on such terms.

Aside from the editor's marvelously succinct and thoughtful introduction and epilogue, which define modernization and summarize the substantive chapters in fourteen propositions, *Political Development in Modern Japan* is organized around seven factors which are presumably central in any process of political development. Professors Hall and Hackett deal perceptively and informatively with leadership: the institution of the emperor, and the Meiji *genro* (the remarkable group of oligarchs who "ran" Japan in her early days of constitutional government). Professor Craig analyzes the cultural content of late-19th century nationalism on the basis of a thorough examination of the interesting evolution of the thinking of Fukuzawa Yukichi, an outstanding philosopher and educator of the time. Professors Jansen and Ike deal, respectively, with Japanese foreign policy and military activity as aspects of the process of modernization; Jansen suggests that several "diplomatic" problems are integral parts of a nation's political development. Professors Steiner and Scalapino describe the halting evolution of popular political participation at the rural local government and the national electoral levels, respectively; Scalapino's study also includes the only treatment of political parties in the volume. Professors Ishida and Silberman take on the concept of differentiation: Ishida deals with the structural and behavioral characteristics of Japanese interest groups (emphasizing their weak organization and lack of autonomy), while Silberman presents a quantitative analysis of the increase in the administrative machinery and tasks performed at the prefectural level of government. Professors Henderson and Tsuji describe aspects of the secularization and rationalization of Japanese law and bureaucratic decision-making; Henderson's chapter is a breath-taking panoramic view of the evolution of Japanese law over the past 125 years. And, finally, professors Ward and Burks analyze the importance of leadership decisions and planning in modernization; Ward's provocative study deals with the postwar Occupation as an "experiment" in planned political change.

The volume is difficult to describe. On the whole it is a comprehensive and competent survey of the subject, and should be very useful as such. Most of the studies focus on events of the early years of modern Japan before the First World War; only Steiner (who contrasts local government in the 1880's, 1930's, and postwar), Ishida, and Henderson attempt to deal with the sweep of developments over the past hundred years. The authors are historians, political scientists, and a lawyer. In general, the political scientists present their studies in connection with a theory of modernization (evolution of political culture in the Almond-Verba sense, mobilization regimes, differentiation, guided democracy, the relative importance of socio-economic versus political factors, etc.), while the historians attempt to resolve issues which have vexed students of Japan (how much and in what ways did the pre-modern, feudal system influence later developments?, "what went wrong" with modernizing Japan to lead her down the path of militarism?, what was the impact of the West?, etc.), but some authors do both. Most of the authors have already published lengthy monographs on the topics of their chapters; these chapters have the quality of a "summary and conclusions" (which can be very helpful for students and for scholars in related fields looking for an overview), and so are occasionally unpersuasive without the omitted documentation. The chapters share an interest in things Japanese which other scholars of comparative politics may have a hard time matching. Nevertheless, because of the organization of the book, the competence of the authors, and the editor's opening and conclusion, the volume does deal with several important general questions. Most interesting, to me anyway, are the issues of "conservative modernization," of the preconditions for modernization, and of the desirability of "modernization."

Most of the authors and the editor are agreed that the modernization of Japan in the Meiji period (1868–1912) was conservative in the root sense of the word. *Given* that Japan was to become economically and militarily strong and politically sovereign and secure, existing institutions, customs, and social class relations were to be changed as little as possible. Traditional structures and ways of thinking were to be renovated: maintained, but altered so as to be useful in the pursuit of the leaders' economic, military, and political goals. That is, the strategy of the Meiji leaders was to modernize while conserving as much as possible. Further, although the authors do not explicitly say so, the *goals* as well as the strategy of Meiji modernization can be seen as conservative—in contrast, anyway, with the goals of 20th century social revolutions. (Ward's analysis of postwar democratization provides a helpful contrast. The Occupation, he says, when it was successful, pursued a strategy of "the creation of new vested interests." The Meiji elite, on the other hand, could pursue a strategy of satisfying existing vested interests as much as possible, since their destruction was not desired nor usually required.) The results of this conservative modernization were national integration, stability, continuity of policy, and of course success. (The four chapters on leadership and participation make this point clearly, as does the editor's epilogue.) Furthermore, many of these authors imply, Meiji conservative modernization did *not* necessarily lead to the "fascism' and militarism of 1931–1945. To the contrary: there was "substantial change and liberalization between 1890 and 1932;" the problem lay, rather, in the fact that "Social institutions or forces that are constructive and functional for one set of circumstances, stage of development or level of input may be destructive and dysfunctional for the modernization process in others." (Ward, pp. 588, 586.) But there is neither agreement upon nor much sustained consideration of this fundamental point. Steiner and Scalapino document the extremely limited nature of popular participation in Meiji modernization, but avoid getting involved in questions of historical inevitability. Ishida does attempt "a statement of the nature of the pattern of modernization in Japan that can explain the whole historical development of Japan, including the Meiji period and the relatively liberal and militarist stages that followed," and Ike ("war represented an integral part of Japan's modernization process") and Henderson ("in the Meiji legal order . . . law became the implement of, but not a limitation upon, legislative power;" various "factors prevented the realization of a justiciable rule-by-law in the administrative field, except to a token degree;" the private law codes "often perpetuated old power relationships in the name of the codes' contractual, personal, or property rights") do see in Meiji Japan some of the seeds of the later militarism and "fascism." The other authors either avoid the problem or implicitly accept Ward's formulation. It is too bad that this massive volume treats this important question so casually.

All the authors who consider the question of the preconditions for modernization are agreed that the process was significantly shaped by conditions in the pre-modern, feudal period before 1868. Hall on the emperor, Craig on nationalism, Steiner on rural political culture, and Henderson on law are explicit about the need to consider arrangements and developments in the earlier period. The arguments given are persuasive. But I question whether anything has been shown about the *preconditions* for or the *beginnings* of modernization. Recall the Japanese who went to Europe and the United States in the years soon after Perry, who sometimes thought that almost everything they saw was an integral part of Western modernity and strength. Only partly facetiously I would suggest that some of the authors of this volume have gone to Meiji Japan and have assumed that almost everything they saw there was part of Japan's modernization. Beginning in this way, they can easily argue that Japan's modernization was "a cumulative product of two and a half to six or seven centuries of gradual preparation." (Ward, 580.) But to show that pre-1868 Japan had a well-worked-out "legal" system of "rule-by-status" which significantly influenced (retarded) later attempts to institute a new, modern legal system (which Henderson shows) does not allow the conclusion that the pre-modern system was a precondition or the origin of the modern system (and Henderson does not make this mistake). Nor does the existence of a traditional institution like the emperor which can be renovated and used in pursuit of modernization mean that that institution was a precondition or a part of the origin of modernization. Just because a plate was in the kitchen while dinner was being cooked should not lead one to conclude that the plate was a precondition or a part of the origin of the dinner, even if the dinner is served on the plate! The problem, of course, is the lack of a theory of the modernization process—a lack which is hardly to be blamed upon these authors.

Finally, the authors raise by their implicit disagreements the question of whether modernization is a desirable thing or not. They are all very careful not to sneak ethnocentric assumptions about the goals of modernization into their analyses. So careful, in fact, that for some their "modernization" becomes a Frankenstein which I wonder whether they value or desire for their own country. (*Vide* Ward: " 'any perfectly modern political system' . . . would obviously be an intolerable monstrosity by any humane standards.") The treatment of popular political participation illustrates this tendency. Once scholars thought of popular participation through elections and self-government as a (or, even, the) basic component of political development. By now, participation has come to mean "involvement with the political system, though not necessarily with its decision-making aspects" (Ward, 7), and trends in bureaucratic control of the countryside and in the electoral fortunes of admittedly elitist parties, described by Steiner and Scalapino, are called "political participation." Development (which is not distinguished in this volume from modernization) has no theoretical link with democracy, nor with progress in any normative sense. In all this, the present volume seems to be in the main-stream of American social scientists' thinking about modernization. The lawyer Henderson is a refreshing and instructive change. His unabashedly normative conception of legal development permits him to identify clearly those concrete changes which are, and are not, "modernization." Social scientists could find a lesson here. It may be more fruitful to use admittedly *a priori*, i.e., normative, models of modernization or development than to continue with putatively empirically-derived definitions. (Such a switch in theory-building strategy might, for example, take care of some of the problems of analyzing "conservative modernization" and "preconditions" noted above.)

This is a stimulating and useful book. It should be particularly helpful for scholars who wish an up-to-date survey of knowledge about political events in Japan in the Meiji period, and for students of Japanese history and political development, but it also contains several chapters which should interest the specialist. In conjunction with the other volumes of the "Studies in the Modernization of Japan" series, it represents a massive contribution to our knowledge about Japan and about processes of historical change.

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Counterrevolution: How Revolutions Die, By JAMES H. MEISEL. (New York: Atherton Press, 1966. Pp. 237. $6.50) and *The Problem of Restoration*. BY ROBERT A. KANN. (Berkeley: University of California Press, 1968. Pp. 441. $15.00.)

Revolution, some contend, is a term politics has borrowed from astronomy. Political life, like the recurring motions of the planets, seemed, to Renaissance man, to follow a determined cycle in which change was inevitably followed by a return, a restoration, to the starting point. This original connotation holds quite well for the English Revolution of 1688, but as we all know this all changes in the eighteenth century and the concept takes on a different meaning. Revolution comes to connote an abrupt break with continuity, indeed a dramatic failure to maintain inevitable patterns. In light of the earlier meaning, however, it is very fitting that the many recent books and articles on revolution and internal war should now be followed by two books on restoration and counterrevolution. Because of their appearance we now know that it is just as difficult to write comparative analysis of reaction as it is of revolution.

No two books on the same subject could differ more than James Meisel's *Counterrevolution* and Robert Kann's *The Problem of Restoration*. More at variance than their substantive positions are the styles of the two books. Professor Meisel's book is a series of rambling reflections, informal, intuitive, generally quite brilliant and self-consciously casual. Little attention is given to a rigorous argument developed and sustained throughout the book. In contrast to this Professor Kann's book is sober, formal, and elaborately reasoned. Anything but casual, his is the traditional method of researched inquiry. Still the books complement each other, and despite their different approaches they are both interesting and important.

Professor Meisel's book is in no sense an investigation of counterrevolution. Neither history nor hypothetical political science, it is simply a collection of brilliant essays (many of which have been published before, which may account for the unstructured quality of the book) on subjects which and people who fascinate the author. These essays are on diverse topics: the

Hebrew prophets, Cicero, General Monk, L'Abbé Sieyès, Louis Bonaparte, Freud, Weber, Durkheim, Pareto, Lenin, Trotsky, Mussolini, Hitler, T. E. Lawrence, Franco and de Gaulle. Behind the innovative features of the book's style (dialogues, memoranda, testimonials, journalistic narratives, personal reminiscences) there are subtle evaluations and speculations about many important events or fascinating individuals. There is, however, little if any detailed investigation of the title subject, counterrevolution. But the reader should not really expect this, for he is told early on that it is to be a book about people, second-raters, who tend to be around when revolutions end.

Despite the risk of giving too much unity to this diverse collection of essays, I think a case for calling this a book about counterrevolutions can be made, since the essays tend to be about one or more of five possible forms of counterrevolution. The first is the familiar variety which Meisel labels "devolution." General Monk, Napoleon, Louis Bonaparte, and Franco are the agents of "devolution." Meisel goes one better than Brinton for whom the end of revolution was the decline of fever and the return of the body politic to reasonable health. For Meisel, revolutions like "tautly wound cable" must literally devolute, roll back from "strains which have become unendurable." A second form of counterrevolution he labels "intercessions," which are "preventive counterrevolutions." There are two varieties of intercessions, bad ones—Mussolini and Hitler—and less bad ones—General de Gaulle. A third species of counterrevolution found in the book is intellectual, that of Freud, Weber, Durkheim and Pareto against Marx. A fourth is existential and personal, thus an essay on T. E. Lawrence. The final variety of counterrevolution one can find is doctrinal and thus the rationale for discussing Stalin and Lenin, and the Old Testament prophets and the modernizing Hebrew Kings.

This unity is imposed solely at the discretion of the reader, for little care is given by the author to structured argumentation. A fascinating book, full of meaty chunks of contentious speculation, *Counterrevolution* is by no means, however, a contribution to the comparative study of either revolution or counterrevolution. Indeed, it was not intended as such.

This cannot be said of Professor Kann's book. Its purpose is to generalize about and find uniformities in the recurring historical phenomenon of restoration. The scope of his inquiry is staggering, revealing a writer learned, judicious and wise. After some one hundred introductory pages of general methodological and theoretical speculation on such interesting topics as time, change, determinism, tradition, continuity, legitimacy, force and violence, he turns to a detailed study of what he calls "the cases." From antiquity he discusses the restoration of the Jewish homeland after the Babylonian exile, the restoration of Greek "democracy" after the rule of the Peisistratidae and the restoration under Marius to pre-Gracchan conditions. A second set of "cases" includes investigation of Justinian's reconquest of Italy, Otto the Great's restoration of Charlemagne's empire, Henry II of England's restoration of the Norman centralized monarchy, Rudolph of Hapsburg's restoration of lost Hohenstaufen imperial authority, and finally the Counter Reformation of late sixteenth century and early seventeenth century Austria. The final category of "cases" consists of the two "classical" restorations, Stuart England and Bourbon France. Each study is preceded by an historical chronology and periodization of these dates into three stages, "the Original System," which he shorthands as (A), "the Intermediate System," (B), and "the Restored System," (C). Each case studied is abstracted to fit this three-fold categorization.

In the theoretical introduction Kann introduces one major hypothesis which he quite successfully sustains in the case studies. "The reestablishment of a system must come about within the life span of a political generation and its endurance must be tested within another one." He also contends that the cases studied establish a "guideline" to help in predicting the success of restorations. They succeed only if they absorb a good part of the ideas and institutions of the intermediate system: (C) must thus be a synthesis of (A) and (B). Restorations reproduce anciens règimes only if along the way they accept some of the innovations of the intervening revolution. The usefulness of this "guideline," he holds, is seen in comparing the French effort at restoration in 1815 with the English efforts of 1660 and 1688. With this few can quibble. But can this be extended beyond the "classical" cases to all the others he has cited? In what real sense can one even compare these "cases"? Some, like the Jewish and the Justinian are territorial restorations; some, like the Roman, the French, and the English, are social and political; some, like the Austrian, are intellectual and theological restorations; some, like the Ottonian, the Henrican and the Hapsburg are restorations of strong central monarchs after crises of succession. Here is the heart of the problem. Is comparative analysis of restorations any easier than of revolutions? Is it not even clearer here that one is comparing apples, and pears, and plums, and that no amount of abstraction by which all the differing fruits are seemingly reduced to some common parts (A), (B), and (C) will make the task any easier to realize?

But Professor Kann is too wise a man, and his book too learned and reasoned a production to be unaware of these problems. His search for hypothetical validation and "guidelines" for prediction is tempered by a modesty surprising in one pioneering a subject, a self-restraint which time and time again finds him cautioning the reader that no social or historical laws are being chartered, not even probabilities, but simply "the establishment of certain analogies common to a number of well-selected cases." Because this motivation operates and because Professor Kann's vast erudition is enlisted to serve that modest end, *The Problem of Restoration* is a truly important and valuable contribution to the study of comparative political history.

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Inside Bureaucracy. BY ANTHONY DOWNS. (Boston: Little, Brown, Inc., 1967. Pp. 292.)

The author set out to "develop a useful theory of bureaucratic decisionmaking"—useful in that it "should enable analysts to predict at least some aspects of bureau behavior accurately, and to incorporate bureaus into a more generalized theory of social decisionmaking. . . ." Bureau, as used by Downs, is a large organization, with full time employees comprising the majority of the membership. Bureaus operate in a non-market environment (output cannot be "objectively" evaluated) and their members are hired, fired, and promoted on the basis of their performance in the organization.

The "theory" is allegedly based on and the majority of conclusions are allegedly derivable from three basic "axioms" or "postulates": (1) bureaucratic officials are intendedly rational. "(A)ll agents in our theory are utility maximizers," given their limited computational abilities and the cost of information. (2) bureaucratic officials have a complex set of goals, some of which can be referred to as "self-interest," "(E)very official is significantly motivated by his own self-interest even when acting in a purely official capacity." (3) "Every organization's social functions strongly influence its internal structure and behavior, and vice versa."

The "theory" consists of the relationships between the following major types of variables: "ten internal dimensions of bureaus (communication system, information-gathering system, distribution of formal and informal authority, resource allocation system, purpose-and-function definition process, methods of and attitudes toward innovation, internal separation of functions, stability, methods of interacting with external environment, and mixture of types of officials), eight structural aspects of bureaus' social functions (clarity with which functions can be defined, ease of calculating bureau effectiveness and results, stability of internal technological environment, etc.), five specific types of bureaucrats ("purely self-interested employees": climbers and conservers, and "mixed-motive officials": zealots, advocates, and statesmen); and certain other normal charactersitics of organizations' and officials' behavior." The book is really about bureaucratic structure and contains relatively little that is unique about bureaucratic decisionmaking.

Downs has succeeded in cataloging much of the folklore of government bureaucracy (e.g., *"Law of Increasing Conservatism*. All organizations tend to become more conservative as they grow older, unless they experience periods of very rapid growth or internal turnover.") and in listing many of the tricks-of-the-trade utilized by Washington, D.C., bureaucrats (e.g., "Successful high-level officials develop whole informal networks of outside information sources through which they can verify the reports made to them by their subordinates.") Because of this, *Inside Bureaucracy* is a valuable contribution to the literature and its insights will be of substantial interest to scholars and students or organization theory.

The sections dealing with the life cycles and growth of bureaus are a mixtue of notions from Weberian sociologists (routinization-of-charisma accounts for birth of bureaus, organization structure reflects division of labor, etc.) and Parkinson ("Work—and bureaus—expands so as to fill the time available for its completion.") as are those parts of dealing with the internal structure of the bureau. The section on the inevitability and necessity of bureaus and other non-market organizations in society presents a lucid, welfare-economic discussion of the appropriate role of government in a market economy. Discussions of decision biases, communications, control processes, hierarchies, organizational boundaries, and bureaucrats' ideologies are a vague mixture of Downs' three basic "axioms" loosely woven in with notions found elsewhere in the literature. The interesting description of organizational search and change is very similar (with explicit consideration of the costs of search and information) to those found in March and Simon's *Organizations* and Cyert and March's *Behavioral Theory of the Firm*.

The major shortcoming of the book lies in the disparity between its theoretical "ambitions" and its actual accomplishments. Although it *might* be true that *some* of Downs' conclusions can be derived from his three "axioms," he does not even approximately demonstrate that. The author's condescending style and methods of reasoning are also sources of some irritation. When coupled with his penchant for coining phrases ("shrinking violet and superman syndromes," "straight-scoop by-pass," etc.) and for creating "Laws" out of whole cloth, a work that might have been described as "creative insight" comes dangerously close to being pompous. E.g.,

> The bureau "customers" of the central search agency will have an ambivalent attitude toward it. They will ask it to furnish all information of any positive value, regardless of cost, since they do not have to pay for it. This conclusion has the following implications. First, no matter how large a data gathering and handling capacity the central search agency possesses, its facilities will always be overloaded. This results from the Law of Free Goods: *Requests for free services always rise to meet the capacity of the producing agency.*
>
> Second, officials of the central search agency will develop nonpecuniary prices for their services. These are devices for imposing costs upon members of other bureaus who request information. They will be designed both to discourage requests and to provide rewards to central search agency members. Such "quasi prices" will include demands for reciprocal favors, long delays, and frustrating barriers of red tape. This illustrates the Law of Non-Money Pricing: *Organizations that cannot charge money for their services must develop non-monetary costs to impose on their clients as a means of rationing their outputs.* Hence, much of the irritating behavior of bureaucrats often represents a necessary means of rationing their limited resources. . . .
>
> Third. . . .

Throughout the book, the "Laws" appear almost magically, not following directly from the discussion, from the original "axioms," nor from any presented empirical evidence. *Inside Bureaucracy,* contrary to the author's claims, does not represent a theory in any sense of the word.

As can be seen in the above example, the book contains strong doses of C. Northcote Parkinson. Unfortunately, Downs' first-second-third-hence-therefore style prevents him from being as entertaining. We are forced to take both his conclusions and his methods of arriving at them seriously.

The author's previous book, *An Economic Theory of Democracy,* was a relatively tightly-reasoned, original, and frequently brilliant piece of work. In it, Downs demonstrated the efficacy of a few axioms and a relatively simple theory involving few variables. In this book he introduces a hopeless number of variables and fails to utilize his "axioms" in any meaningful way. At the end of the highly-successful *Economic Theory of Democracy,* Downs listed a set of "testable propositions derived from the theory." He saw fit to repeat the practice in this book. In the *Economic Theory of Democracy,* it took considerable effort to discover that the propositions were neither testable nor derived from the theory. In this book there is no theory and the non-testability of the propositions is immediately obvious. If nothing else Downs has demonstrated how essential it is to empirically test theory and to use operational concepts in theory construction.

In spite of the book's considerable shortcomings, it also possesses condierable virtues. Foremost among them is the catalytic effect *Inside Bureaucracy* will have on its readers and on their research. The nineteen pages of axioms, laws, and propositions are packed with interesting and significant research questions and bureaucratic folklore. The space devoted by the reviewer to the virtues is by no means a measure of their relative importance.

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Political Parties in Western Democracies. By LEON D. EPSTEIN. (New York: Frederick A. Praeger, 1967. Pp. 274. $8.50.)

Leon Epstein's *Political Parties in the Western Democracies* will almost certainly become a standard element of the literature on political parties. It is a book informed by long and very cautious thought, and by a very thorough canvass of the existing literature. It is a book which will revise the received wisdom at several points, though not a book which will altogether reorder the way we think about party politics. It is certainly a book all of us should own and read.

In many ways, Epstein's book stands Maurice Duverger's very well-known work on its head. For Duverger, politics turns on large determinicies; for Epstein, politics hinges on a collection of not so grand contingencies. For Duverger, parties develop toward an idealization of mid-Century continental working-class parties; for Epstein, they seem currently to be progressing (or regressing) irregularly toward the model offered by American middle-class parties. Perhaps the difference is between a European and an American scholar; perhaps it is the difference between a rationalist and an empiricist. In either case, Epstein quite consciously takes aim on the Gospel according to Maurice.

A central example is offered by the question of changes in party organization. Duverger thought he saw a determinate progression from brackward cadre-based parties to advanced mass-based parties. He went on, of course, to coat this progression with a naturalistic sense of goodness, making the mass-party appear "advanced", the cadre-party "backward". It followed that middle-class cadre-parties would learn to imitate the working-class mass-parties, or face extinction—a process which he called "contagion from the left".

Epstein argues that Duverger mistook an admittedly important set of early 20th Century contingencies in European history—class-consciousness and socialist unionism, prebroadcast technology, and rapid extension of mass suffrage—for a universal determinacy. Since one cannot expect these contingencies to retain a single exact configuration, one cannot expect the alleged determinacy to persist.

Indeed, Epstein puts forth the thesis that a new series of contingencies—centering about television and its depressive effect on the importance of volunteered labor and mass dues financing—may have set an opposite (though perhaps temporary) trend in motion. Now, expertise in the use of media and big money are relatively more important than mass canvassing and masses of small contributions. The result of this reversal, according to Epstein's argument, is "contagion from the right." Parties which Duverger thought backward—especially the major American parties—have learned to use the medium earlier and more effectively than Duverger's advanced mass parties, and the latter are just beginning to imitate these backward cadre parties as they themselves are forced to rely increasingly on television, and not on activists, to reach the electorate. The upshot of the argument, of course, is that Duverger's linear theory of party evolution is reduced to a quite ordinary, partly incorrect hypothesis. It is not so much that Epstein has proposed a revolutionary new "theory," but that he has revised and scaled down an old hypothesis.

It would, indeed, be very wrong to think of Epstein's book as a fundamental revision of the field. It does, as we have seen, often arrive at new conclusions. And it does, as I have already said, demonstrate a special and attractive approach to party politics. But its new conclusions follow from established questions, and its approach is well within the confines of conventional political science. Epstein takes strong (and insightful) exception to much within the conventional literature, but does not take exception to the categories which organize it and the questions which move it.

A major portion of the essay, for example, is organized by a very informal, very eclectic version of functionalism. Functions, here, are both things parties do for political systems and payoffs parties provide for their clients. The former (macro-) strand of functionalism is not in Epstein's (or any other) hands a powerful explanatory device. The later (micro-) strand of functionalism is potentially more interesting, but would have profited from a closer consideration of the equilibrium concepts proposed by Chester Barnard and Herbert Simon some time ago. There is nothing theoretically new in the use Epstein makes of functionalism.

The other major set of organizing concepts are drawn from democratic theory, and these are to be recommended at least for their straight-forwardness. Epstein divides the democratic norms which scholars have brought to the study of parties into three groups—majoritarian, pluralist, and individualist versions of democracy in large communities. These are helpful, for they enable Epstein to avoid the fruitless question which plagues a large sector of this and related fields—"What does 'democracy' *really* mean?" If the book does not remake the place of parties in democratic theory, it is at least unusually clear about the place others have given them.

A major virtue of the book is its serious attempt at genuine comparison. Despite what Epstein quite frankly calls his "Americanist bias," the book is organized by the theoretical categories we have been discussing, not by the units of political geography. The U.S. (and Great Britain) may receive disproportionate attention, but this imbalance cannot be blamed on Epstein. And, even if the theoretical categories are not revolutionary, they are at least lucid. And they offer greater coherence by far than do the country-by-country patterns of organization so common in the literature on parties.

And Epstein must be praised for the unpretentious, lucid style in which he writes. Keeping in mind the many garbled definitions that have been offered for the term 'political party,' Epstein's cannot fail to refresh the reader:

> It is time to define what is meant by political party. I have no desire to depart from common understanding. . . . [A party is] any group, however loosely organized, seeking to elect governmental office-holders under a given label. Having a label (which may or may not be on the ballot) rather than an organization is the crucial defining element. (p. 8).

We may find this definition useful for some purposes and not for others, but at least we can understand it, and so can our students. This straight-forwardness is characteristic of the entire book.

This frank reliance on common prose does, however, associate itself in many books, including this one, with a failure to use statistical and mathematical approaches where they are needed. Many problems in party politics are most effectively (and intelligibly) met with mathematical or statistical thought. But Epstein's book uses very little beyond the presentation of tabular data. Discussions involving the character of inter-party competition, for example, are not at their best where no numerical (much less mathematical) analysis is offered. It is my impression that this omission does not so much misguide Epstein's analysis as it limits the range of questions open to his attention.

Seen in the larger perspective, then, this is a fine and often sophisticated book. It does not offer a sort of post-Duverger renaissance to the study of party politics, but it does offer important revisions within a continuing tradition. It is insightful and pointed. It is well-written and unpretentious. It is, in language as direct as Epsteins own, a good book.

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A Great Society? Edited by Bertram M. Gross. (New York: Basic Books, 1968. Pp. 362. $8.50.)

Few Americans, it seems safe to say, regard seriously the tags which Presidents fashion for their administrations. Often these tags are as much the choice of the press as of their formulators and are notably inept, The New Deal being a prime example. Just what President Johnson had in mind in his choice of label is far from clear, but Bertram M. Gross has paid him a notable compliment in taking that choice seriously and making it the title of a book of essays by a distinguished group of scholars. Professor Gross and some of the contributors make efforts to define the phrase, but they are more successful in discovering a variety of antecedents than in reaching agreement on a single meaning. Understandably, some of the best essays simply ignore the problem.

There is something rather mournful about the appearance of such a book when the term's progenitor has withdrawn from the partisan battlefield and whatever dream he had has dissipated. But there are other ironies in the venture as well. While the title in one of its plain language meanings would suggest a *large* society, the apparent theme of the editor and some of his contributors is a call for decentralization and return to a world of small societies. This call is further associated with a doctrinal affirmation of the pluralism paying tribute to the name of Arthur Bentley. Certainly Bentley contributed to the breakout of political science from its one-time formalistic confines, but that battle ended long ago, and the critical movement of the present is a similar escape of the discipline from the narrow confines of the pluralism which has been the orthodoxy of the last few decades.

If the book as a whole suffers from the dilemma of being too late to give guidance to a one-time powerful administration and too early to be able to assess that administration objectively and if its intended theme looks backwards, it is yet a highly honest and useful volume. The editor is far from insensitive to the discriminatory effects of the pluralism he idealizes and some of the contributions in the book contain sharp criticisms of that doctrine's implications.

The most incisive of these criticisms is the article by Norton Long, "Local and Private Initiative in the Great Society." With his customary keen sense for the relevant, he looks to the problem of the cities in American life. Here, the idealization of small units is absurdly at odds with the goals of greater equality and decency of life in the settings in which most Americans now live. This is as true in the fragmented governmental units of suburbia with, in his wonderful term, its "toy governments," as it is in the inner city where the neighborhood is being told its salvation lies in its own hands. Autonomous "self-government" and "participatory democracy" are, as always, OK words and they evoke a pleasing picture of a New England town meeting, but neither suburban "village" nor ghetto neighborhood is an island and it is time that both realized they are part of the main. The cold hard facts of tax resources, if nothing else, should be proof of that reality. Although Professor Long only hints at it, the suspicion seems justified that some of the ready acceptance of these ideas of autonomy and self-help rests at least partly on an unacknowledged understanding that these are the keys to maintenance of discrimination and privilege.

Although the book opens with a sympathetic presentation of President Johnson's own reflections and desires for the Great Society, it is notable that it also contains statements by perhaps his angriest critics, Herbert Marcuse and Hans J. Morgenthau. These articles are highly convenient summations of their positions, familiar as these have become. To Professor Marcuse the Johnsonian program is ambiguous, but also evil, being built on such American values as "commercialization of the soul, togetherness, the sanctity of business, the science of human relations." In its warlike neo-colonialism and general crassness, the America of President Johnson gives no chance for individualism or any of the decencies of a *real* Great Society. Professor Morgenthau says briefly what he has repeated with marked effect elsewhere: that the President has mistakenly and dangerously sought consensus where he is only entitled to consent, and the latter presumably only for different policies than he has followed, particularly in foreign affairs.

Some of the most interesting passages in the book are the assessments of what changes are overtaking American government and politics. Thus Daniel Bell in a discussion deceptively presented as a collection of notes argues that the vision of American politics descending from Bentley through David Truman and others is "astonishingly out of date for an understanding of politics in the second half of the twentieth century," failing to take account of the decisive charactersitics of policy today: "the influence of foreign policy, the 'future orientation' of society, and the increasing role of 'technical decision making'." He notes the blurring of the line between public and private organizations and finds great significance in the cooperation exemplified by RAND and its imitators.

Peter Drucker also perceives a deep change in American politics. To him the old economic issues of politics and interest group contests and the consequent patterns of accommodation are becoming obsolete. Economic policy, he claims, has now become a technology rather than a philosophy and domestic politics seem "likely to shift to a concern with the basically political—that is, with constitutional, moral and esthetic issues," with a resulting new preoccupation with power. There follow, he says, a passing of Southern political power, an erosion of economic blocs, reduction of Congress to the status of critic and commentator, and trouble for the parties, especially the Republican. With Professor Bell he stresses the interlaced character of private and public organizations but goes a step further to see in the commingling of corporations, labor unions and governmental bureaus a "new federalism." In a broad sense this is not too different from the "military-industrial complex" so troubling to radicals. Professor Drucker, it may be said, is bravely undismayed by his discovery.

The book, then, is an accurate and able reflection of a considerable part of the spectrum of social and political discussion occurring in the contemporary intellectual and semi-popular press. Despite some editorial effort to eulogize Arthur Bentley and to press the case for the sort of federalism variously seen as "new" or "creative," there is ample statement of the difficulties in maintaining such retrospective visions. Despite the misfortune of its title, it will be a useful volume to have available through the uncertain years in which we seem to be midpassage.

GRANT MCCONNELL

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Law Without Sanctions: Order in Primitive Societies and the World Community. BY MICHAEL BARKUN. (New Haven: Yale University Press, 1968. Pp. 179. $6.50.)

Michael Barkun's book is a welcome contribution to those scholars who distinguish between training in law and the study of law, and who are concerned with the worth of the latter in advancing legal scholarship. This is a suggestive book, pregnant with ideas and propositions, constituting a firm step toward the scientific study of a field that has for many years been the province of practitioners. Barkun undertakes an attempt to overthrow, or at least to lessen the inverse relationship between the existence of a developed profession and the development of pure scholarship in the substantive area of law. It is a fruitful undertaking even though it does not come up with all of the answers, or even perhaps with any of the ultimate answers to the long-standing problems of legal theory that are the legacy of John Austin. As a germinal book in legal theory it poses some very intriguing propositions and suggests some very seductive paths of inquiry in developing the scholarly area of the study of law as an important segment of human experience which crosses the disciplinary lines of jurisprudence, political science, sociology, and anthropology.

The social and political role of law is an area of research that can offer great insights into human behavior, especially in the areas of activity that interest political scientists. As Barkun points out, "Law provides a means for maintaining change within humanly acceptable limits, for allowing innovation by increments while keeping the general scheme of things within accustomed tracks—for perpetually altering the status quo while perpetually preserving it."

This book is a real contribution to the theory of law which uses as its examples the two main social contexts in which Austin's command theory of law fails to provide a rigorous, or even applicable theory: primitive societies and the world community. Barkun points out the paralyzing limitations of the Hobbesian-Austinian command theory of law in the these contexts, not the least of which are the research aims that are limited by thinking of law in terms of the command theory of law in these contexts, not the superfluous task of a major demolition job on the command theory. Barkun's point of view is that of sociological jurisprudence as fathered by Eugen Ehrlich and restructured in twentieth century America by such men as Holmes, Cardozo, and Llewellyn. Barkun, however, goes beyond Ehrlich's generalities about the "facts of the Law" or Llewellyn's exhortations to examine judicial behavior, or F.S.C. Northrop's idea of the "living law", to suggest some general theoretical propositions about law which are applicable to societies with or without political or legal structure in the formal sense. He also offers a more useful theoretical approach to the empirical political scientist than H. L. A. Hart by basing his observations on general empirical observations. Moreover, Barkun's work has the admirable quality of suggesting substantive theory rather than remaining in the penumbra of a pre-theoretical or methodological contribution.

Any general theory of law (and at the present time there really is none fully adequate) must cut across cultures rather than being bound to a single arrangement of institutions, must encompass rather than exclude present deviant cases, must cut across disciplinary lines when there is reason to believe that these lines have become outmoded as research guides, and must be confirmed in observed patterns of behavior. Barkun suggests the way to this goal. The empirical referents for this theoretical undertaking are those legal systems which remain on the margin of the classical approach to jurisprudence: international legal systems and primitive legal systems.

The Nuer of the southern Sudan and the Tiv of western equatorial Africa are examples of primitive societies that are jural communities examined by Barkun, and the international counterpart of these primitive communities are the different international systems that have existed in the past several centuries. The function of law within these primitive societies and the world community is essentially mediative, wherein conflict situations are resolved by turning a dyadic conflict relationship into a triadic relationship through the mediative element that is law. In modern developed states legal conflict resolution takes place in an explicit mediative fashion through complex sets of normative rules which are applied by sophisticated administrative institutions, completing the triadic conflict relationship. In primitive societies and the world community this mediative function is also performed, but the law-mediator is only implicit, and invisibly present in the form of system preserving values which have for the most part developed out of precedent settlement of conflict situations.

In the latter two examples of jural communities the systems are characterized as being "horizontal" rather than "vertical" systems of law, where the law is "handed down" from the sovereign to his subjects, a distinction that has often been used to distinguish the international legal system from the systems of municipal law, particularly by Richard Falk. In horizontal systems, however, there is nonetheless a discernible structure through which the legal process takes place. Within the international community and within the primitive societies there are patterns of behavior and social structure which permit "legal" conflict resolution to take place in the triadic, mediative fashion. The segmented lineage systems of primitive societies and the present bipolar structure of the international system are examples of the types of structure which define and limit conflicts, and which provide the actual source of the invisible mediator. The illustrations used by Barkun in demonstrating this proposition for primitive societies are drawn from anthropological literature. His use of the literature is selective and will for the most part be unfamiliar to the political scientist. On the other hand his referents for propositions dealing with the world community may be found to be a little too general and lack rigor for the scholar of international politics and international law. This criticism is not devastating, however, for Barkun seeks less to weave a tight argument around these specific examples than to provide the broad outline of general legal theory.

Barkun convincingly and effectively develops the isomorphism of law in the primitive society and the world community based on the implicit mediatory role of law in conflict resolution, and in doing this he goes beyond the simple development of this isomorphism to treat other relevant and penetrating questions concerning the nature of law. It is perhaps in these corollary questions that the most suggestive parts of the book lie. Although it is sometimes difficult for the reader to tie all the thoughts together in a single thesis (and Barkun warns of this difficulty), there is sophisticated treatment of such questions as the social basis of law, law and perception, and the relationship between law and societal complexity. Political scientists, particularly those involved in international law or law in "non-modern" societies will find much to interest them in this treatment of normative sources and regulation of behavior in situations where the command theory of law can provide no useful analysis.

The usefulness of the type of theorizing that is done in this book lies in its treatment of law as a political and social phenomenon which has as its main purpose to permit human actions to conform to predictable patterns so that contemplated actions can go forward with some hope of achieving a rational relationship between means and ends. In this context the specific nature and function of law is elaborated. Barkun lists a half dozen "legal univerals" that are found in all societies:

1. Law is a set of interrelated symbols.
2. Legal symbols have some empirical form.
3. The symbols can be manipulated and can be arranged in propositional form.
4. The legal process involves the correlation of fact-situations and normative outcomes.
5. Law as a symbol system is a means of conceptualizing and managing the social environment.
6. The legal process involves the transformation of dyadic interactions into triadic interactions.

The elaboration of these formal characteristics of law provides the basis of theory which can be applied to real-world situations in such a manner as to permit a fuller understanding of law in societies that are not characterized by the existence of a sovereign. Beyond these principles Barkun elaborates additional polymorphous features of primitive systems and international systems which provide much stimulation for thought and should provoke further study along these lines.

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Voting Patterns of the United States Supreme Court: Cases in Federalism 1889–1959. By John D. Sprague. (Indianapolis: Bobbs-Merrill, 1968. Pp. 164. $6.50.)

This is a well-written and sophisticated book which should be read closely by students of the judicial process as well as by specialists in research methods and techniques. The manuscript first appeared in the form of a doctoral dissertation, and received the Edward D. Corwin Award of the American Political Science Association for 1966. In its new and revised form it seems destined for additional—and merited—recognition.

The purpose of the study was to evaluate two different methods of ordering and interpreting voting patterns of Supreme Court justices. The methods chosen for evaluation were *bloc analysis* and *Guttman-Schubert scalogram analysis,* and the data consisted of all non-unanimous "federalism" cases decided by the Supreme Court from 1889 to 1960—a total of 816 "usable" non-unanimous decisions.

Sprague found that for this sample of cases the Justices exhibited several identifiable patterns of association. Rates of division varied so significantly, from only 9% in 1915 to 70% in 1952, that comparison was sometimes difficult. Nevertheless, he was able to identify voting blocs in 28 of the 30 "courts" (defined as the time between personnel changes) which sat during this period, and a total of 54 blocs. Of these, nearly half (25) were of the two-judge variety, and only seven consisted of as many as five justices. From these data, Sprague concluded that "blocs are not relatively permanent subgroups across all cases, but rather are shifting subgroups cued into existence by particular issues." (p. 144) This assertion varies substantially from previous observations, by Ulmer and others, which held that decision-making on the Supreme Court was largely the result of interaction by "cliques" or persistently cohering subgroups.

Sprague's dissatisfaction with current definitions extends beyond his empirical results to the methods normally employed for identifying such groups. He feels that there is no acceptable criterion for arranging justices in a bloc matrix. In part, the problem is one of definition. Sprague points out that one has no assurance that a "bloc" identified by current methods will exhibit "over-all transivity; e.g. we have no assurance that if A is highly associated with B, and B is highly associated with C, then A will also be highly associated with C." (p. 31) This is because bloc matrices are constructed by building *pairs,* but interpreted *across* pair lines. The result is that many three, four, and five man "blocs" are frequently mere illusions of the matrix rather than real-life voting combinations.

The author handles this problem in two ways. First, he devises a scheme of ordering the justices along a matrix which at least attempts to meet the problem of relating A to C. Second, he adopts a very restrictive definition of bloc as "only a subgroup of justices for a given interagreement table [which] meets the criteria. A bloc is a name for a pattern of scores in a summary voting record, no more." (p. 56) While I think that his new rules for ordering justices along a matrix are intelligently conceived, I am bothered by his redefinition of bloc, for two reasons. First, it seems to me that even this definition does not entirely satisfy the transivity problem. There is still no assurance that justices identified as belonging to the same bloc *vote together as a bloc*. All it assures us is that their voting patterns are related. Second, to describe a bloc as merely the label affixed to a pattern of votes devitalizes a concept whose strength must lie in its sensitivity to the *actual* interactions among justices. Furthermore, since Walter Murphy was undoubtedly correct when he observed that merely knowing accurately that judges voted together was a rather weak indication of their interaction or "bloc" tendencies, the vulnerability of bloc analysis becomes even more apparent in the light of Sprague's findings. For the weakness inherent in relying solely on votes to establish bloc identities is only compounded when the accuracy of identifying voting blocs in this way is also brought into question.

Sprague's second approach utilizes Guttman-Schubert scalogram analysis. Unlike his bloc analysis, which produced identifiable subgroups in virtually every "court", his attempts to scale these 816 cases was a failure. This could have resulted simply from a lack of unidimensionality in his subject, or it could have resulted from deficiencies in the techniques and theory of cumulative scaling. Since Sprague used the same definition of "court" for his scaling as for his bloc analysis, he reasons that his inability to achieve scales was due in part to the extended periods of time involved. Most cumulative scaling done by others has been for single terms of the Court, or for relatively short time periods, and Sprague suggests that while "cumulative scaling is descriptively useful in the short run, [it can be] theoretically misleading in the long run." (p. 150) If, as Sprague suggests, voting behavior of judges changes over time as a result of their on-the-court experiences, and that these changes are in the direction of a reduced simplicity of outlook, then he has described a major weakness of cumulative scaling of judicial decisions—the inability of the method to comprehend longitudinal or developmental dimensions.

If this book lacks anything it is a modicum of descriptive background. One might want to know something more about the 816 cases in the sample, or about the impact of these judicial voting patterns on differential policy outputs—not enough to turn the book into a narrative description or collection of anecdotes, but just enough to give greater life and vitality to the subjects of the analysis. But perhaps herein lies a different book—hopefully a sequel to this excellent study.

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Man and His Government: An Empirical Theory of Politics. BY CARL J. FRIEDRICH. (New York: McGraw-Hill, 1963, Pp. 737.)

An Introduction to Political Theory: Twelve Lectures at Harvard. BY CARL J. FRIEDRICH. (New York: Harper and Row, 1967, Pp. 182. $5.50.)

Reviewership has been defined by Stephen Potter as the art of conveying the impression that the reviewer knows more about a subject than the author who has just written a book about it. In the case of this author and this reviewer, this is impossible.

Political science, like many other fields of knowledge, has grown far beyond the point where all of it could be carried in the head of a single man. Even today, however, some scholars come closer than others to this now unattainable degree of comprehensive knowledge of their general subject; and a few of them also have the intellectual power and self-discipline to organize the very great amount they know.

To the best of my own limited knowledge, Carl Joachim Friedrich knows more political science than any other man now living. This is particularly true in the great field of classic political theory and philosophy, but also in the no less important field of political experience as embodied in the comparative study of governments and of political and legal institutions. In addition, he has made distinguished contributions to the study of history; to the theory and practice of federalism and of constitution-making; to the early study of public opinion and of political parties and movements; and to the hard-headed and penetrating analysis of foreign policy and international affairs.

For every topical specialty, and for every particular country or geographic area, there may well be by now some specialists with a greater knowledge of detail. But if the intellectual unity of political science continues to be an important asset of our profession, and a basic condition for its sustained growth as a coherent field of scholarship and social science, then the broad, comprehensive competence and contribution of men like Carl Friedrich, Quincy Wright, Harold Lasswell and a very few others are among the best resources we have.

In *Man and His Government,* Friedrich attempts to put a very substantial part of the essentials of his knowledge into a single book. In his much briefer *Introduction to Political Theory,* he uses his knowledge only as a background for his selection of theorists and topics, and for the questions and judgments he puts forward. Both books are very much the product of a single mind—a mind that has imposed unity and order upon a vast, lively and unruly subject. The *Introduction* is an excellent book for its purpose. It combines great teaching skill with deep thoughtfulness about abiding problems, and with an awareness of the realities of present-day politics. *Man and His Government* is a very different kind of book. It is an excellent instrument for teaching the more mature and thoughtful students from the junior and senior undergraduate level upward, but it is also apt to teach the faculty—all of us—for many years to come.

Man and His Government is the most impressive summary I know of the great tradition of political science within the compass of a single volume. Almost every one of its 675 pages is rich in thought, in facts, and in references to some highly pertinent works of political science, history, philosophy, or the social sciences.

This does not mean that readers will always agree with Professor Friedrich's views. Thus his definition of science (p. 6) seems to me too heavily weighted on the side of the consensus of the current generation of specialists within each currently accepted special field, and it may not stress sufficiently the verification by reproducible evidence from nature or external reality, which may eventually override the objections of the earlier specialists, or which may convince their successor, so as to change some of the basic concepts and methods of a scientific field, or change the field itself, or add a new one—as has happened from Galileo's telescope to Planck's quantum concept and the emergence of such new fields as nuclear physics and radio astronomy.

Again, we may feel uneasy when we read in the chapter on "Political Representation and Responsibility" that "the more permanent minorities who have no chance of ever electing their own choices, because of . . . racial or other permanent differentiation . . . are nonetheless represented but by unasked for representatives who represent them because they exist and occupy the position they do." (p. 308) Or we may wonder whether indeed in any near future "the comprehensive cultural wholes seem to be emerging as the components of this future international system . . . as the term 'nation' is broadened to cover such vast cultural complexes as India, Europe or Africa." Later in the same paragraph" "Latin America" and "Arabia" are seen now as such "emerging" political units, while "much uncertainty surrounds the British Commonwealth." (pp. 673–74)

Even where one feels some reservations about such points as these, the arguments in which they are embedded in Professor Friedrich's are deep-probing, rich and fruitful. One can learn more from disagreeing with one of his arguments, and from trying to think it through in the process, than one could learn from nodding in agreement with some lesser writer.

Most of the time, however, readers will agree with judgments which will strike them as sound and deep at the same time, adding unexpected points and perspectives to what had seemed familiar problems, and opening new prospects upon them with subtlety and sophistication. Among the most valuable parts of the book are the discussions of political community, not only in Part IV, but also in many of the earlier chapters. Another valuable part, rare in books of this kind, is Chapter 21, on "Political Innovation and Invention." Still other strong points seem to me the rich and thorough discussions of "authority," "legitimacy," "leadership," "influence and the rule of anticipated reaction," "justice," "order"—all matters that too often remain vague, or else are forced into small Procrustes beds of one-sided attempts at operational definitions. To be sure, Carl Friedrich does not make many of these concepts fully operational—often he leaves the reader "merely" with a deepened sense of insight and of recognition—but anyone who could try to develop adequate operational definitions for such matters should take very careful note of his work in this volume.

Despite its comprehensive and systematic character, there are certain kinds of political science which are absent or underrepresented in both books. Both works are first and foremost books of thought. Neither of them contains much in the way of specific materials or explicit evidence from such modern methods of political research as survey research, content analysis, voting studies, or the quantitative analysis of aggregate data of other kinds of actual behavior. Sometimes mathematical formulations are offered which reach beyond the present stage of our knowledge, such as a "formula for community" (p. 145), whose particular shape cannot now be proved to be correct or necessary, and whose terms cannot as yet be given quantitative meaning.

If one contrasts this with the extremely rich and sophisticated treatment of the philosophic contributions of Aristotle, Plato, Hobbes, Kant, and Hegel to the tradition of political thought, one might get the impression that Carl Friedrich's thought in both books is primarily oriented towards intellectual history and to the past. The scant index provided by the publisher for *Man and His Government*—only 3 per cent of the pages of the volume—tends to enhance this impression: it lists 55 references to Aristotle, 48 to Plato, but at most two to any political theorist now living in the United States, while the names of such men as Gabriel Almond, Robert Dahl, David Easton, David Truman and Quincy Wright do not appear in the index at all.

This impression, however, would be quite misleading. It is precisely one of the strengths of *Man and His Government* that it does confront the traditional wisdom and experience of political science with the work of modern theorists. This confrontation and discussion is carried on, almost on every page, in the footnotes (which, it seems, were not included in the index). Here modern works and authors are cited, criticized, agreed with, or questioned, in a continuing scholarly discussion that alone would be worth the price of the book. A better index in a future edition would make this valuable material more readily accessible to students and scholars. The present poor index is balanced, however, by a rich and uncommonly good bibliography, which again brings traditional and modern works together.

Altogether, *Man and His Government* is a monument to the enduring intellectual unity of political science. It offers a major contribution of the kind we need now and for many years to come. If it stresses in its substance the analysis of such concepts as "authority," "legitimacy," "hierarchy," "bureaucracy" and "order," it also deals extensively with problems of revolution and of change, and it is particularly well-informed and constructive on the problems of democracy, law and constitutional government. If its methods include prominently the approaches of philosophy and history, its discussions—both in the text and in the footnotes—confront these time and again with the findings of empirical research in the behavioral sciences. I know of no other book that has gone so far in laying the foundation for a genuine and sustained dialog between adherents of the great tradition of political thought and the empirically and quantitatively oriented students of political behavior. It is rare for one book to fulfill the tasks of a small special library. This one does; and no serious political scientist should be without it.

Compared to this massive contribution, the function of the slender *Introduction to Political Theory* is largely pedagogical. It is indeed an excellent introduction. Much of the material it offers is traditional. It stresses most heavily the ideas of Christianity, Plato, Aristotle, Hobbes, Socrates, Marx, Machiavelli, Kant, Hegel, Engels, Thomas Aquinas, Rousseau and John Stuart Mill. References to any recent or living theorists are rare. But the problems to which the resources of the great tradition are applied are modern, and both the ideas and their manner of presentation are thoroughly alive. (The Harvard undergraduates in their unofficial *Crimson Guide to Courses*, usually quite critical of their faculty, called a typical lecture by Carl Friedrich "a tightly organized gem.")

This slim book of twelve lectures lives up to this standard. At the same time, it sets a standard of our teaching of beginners. For it demonstrates the kind of background knowledge which beginning undergraduates should acquire as early as possible in order to approach modern problems of political research and analysis with some of the intellectual and historic depth which their adequate exploration and solution is likely to require.

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NOTES

POLITICAL THEORY, HISTORY OF POLITICAL THOUGHT, AND METHODOLOGY

The Liberal Politics of John Locke. BY MARTIN SELIGER. (London: George Allen & Unwin Ltd., Pp. 372. 55s.)

Perhaps the good deeds of great men are 'oft interred with their bones,' but such is rarely the fate of their ideas, which seem capable of an infinite number of resurrections. Lockean political philosophy is the current beneficiary—or victim—of this resuscitation ritual. That it is experiencing an 'identity crisis' is a mild understatement. We have discovered the 'conservative' (Strauss, Cox), 'bourgeois' (MacPherson)—even the 'radical' majoritarian Locke (Kendall). Professor Seliger's contribution is a modernized version of the 'liberal' Locke. His objective is to provide a "clarification of basic ideological issues by elucidating the components of a political tradition" (p. 26) i.e., liberalism.

Since part of our ideological confusion is traceable to its muddled source, any 'basic' clarification ought to present a reasonably coherent account of Lockean thought. But, even if we understood Locke's views, we would still want to know their relationship to the 'tradition' of liberalism, which, at a minimum, demands a defensible (historical) definition of liberalism. Neither requirement is met in this book.

Was Locke a liberal? Seliger offers no definition of liberalism, but it appears to include political guarantees of individual liberties (p. 21), representative institutions (p. 179), the sharing and limitation of political power (p. 293), and supremacy of law and the legislature (p. 369). Yet, we are told that Locke's concessions to "authoritarian leadership" are "fundamental to his political theory" (p. 24), that he justified imperialism and slavery (p. 116ff.), and the exercise of "absolute power in politics." (p. 264ff.) He legitimized 'forcibly seized' political power (p. 253), was not a laissez-faire liberal (pp. 165, 179), and certainly not a democrat since he opposed majoritarian suffrage (pp. 253, 288).

If this seems a jaded picture of a 'liberal,' it is no less modern for that. For, "operative democratic theory" is "not as far removed from Locke as it pretends to be." (p. 301) For a 'liberal tradition' opposed to universal suffrage (p. 19), fundamental issues reduce themselves to "the procedure through which an elite attains its special rights." (p. 293) Hence, politics is "a continuously competitive process of maintaining . . . a tolerable balance between powers which represent individual rights and capacities." (p. 259) How reassuring to know that "economic proficiency determines in truly liberal fashion the extent of political rights" (p. 292), that 'liberal' regimes may dispense with the "constitutionally expressed consent" of the people (p. 293), that, in fact, the apathy of the masses is "a civic virtue." (p. 320) At last, we see that, for liberals, "the chief executive is at the centre of it all, playing a decisive role in the legislative process and possessing the constitutional authority to act . . . as the sole and authoritarian representative of the will and interest of the people." (p. 367) The true sons of liberalism are Dahl, Laswell, Kaplan, and Easton, who Seliger cites in support of the above statements. Since I have my own reasons for agreeing with this conclusion, which have nothing to do with Seliger's book, I will leave it to exponents of liberalism to consider whether this 'defense' advances their cause.

A few words, however, must be said on Locke's behalf. Whatever his reputation as the archtypally ambiguous thinker, nothing Locke wrote is as confusing as Seliger's 'elucidation' of it. Thus, the 'state of nature' is: "not a real situation" but a "hypothesis" (pp. 82, 90, 258), a "specific situation" within political society (p. 259), "both" a hypothesis and "a real situation" (p. 169), not to mention a "psycho-sociological model" and "socio-political-key-concept." (p. 261) Perhaps it is a self-projected image when Seliger portrays the political ruler as 'hovering' like some giant moth between the state of nature and political society. (p. 368) Under a barrage of misspent verbosity, the problem of 'consent' evaporates before our eyes. According to Seliger, "express" consent equals "tacit" consent (pp. 226, 275), equals "an attitude . . . embedded in a process" (pp. 224–5, 261, 298, 301), equals what men would give *if* they were asked (p. 224), which equals "the absence of active dissent." (pp. 253, 261, 293, 297)

Such leaps of logic are made with the ease of an olympic hurdler, though Seliger ungracefully attributes them to Locke. After drawing a distinction between 'rebellion' and 'revolution,' which Locke did not make, Seliger accuses him of using the two terms inconsistently! (p. 317) A similar point is made in his discussion of 'absolute' and 'arbitrary' power. (p. 245ff.) If, says Seliger, rebellion is opposition to authority, not persons, then "it follows" that revolution is opposition to persons, not authority. (p. 317) Or, "any regime must be regarded as sustained by consent until it is challenged by revolution." (p. 298) Defending his interpretation of Locke's advocacy of one-man rule, Seliger's comment on a seemingly democratic passage ("it was the Tribes and the Congregation, that debated, resolved, and directed all that was done . . ." *First Treatise,* par. 165) is that it does not exclude individual leadership. (p. 220) On that reasoning, rule by a Supreme Court or a Poohbah cannot be excluded either.

Although Seliger proposes to discuss only the "mature" Locke, he makes no use of the "mature" works on toleration and education (when will we learn that not even a political theorist says everything in one book?), and quotes unsystematically from two early works to confirm some of his arguments, ignoring or rejecting them when they disconfirm others. Given Seliger's emphasis on Locke's "revolutionary" views, his non-developmental approach is particularly ill-advised, since, for most of his life, Locke was not a revolutionary; and, in his *Journal,* only three years before beginning the *Two Treatises,* he reaffirms his non-revolutionary position. Thus, despite its importance, Seliger offers no insights whatsoever into the intellectual, social, and political reasons for Locke's change of heart.

This exposes another difficulty: viz., in addition to inattention to Locke's other published works, Seliger writes without a knowledge of the approximately thirty volumes of Locke's correspondence, journals, notes, and manuscripts, not to mention contemporaneous writings and other historical evidence one might have used as aids for understanding some of the problems of Lockean philosophy. The apparent excuse, that history provides little guidance because contemporaries are themselves embroiled in controversies (pp. 32–3), would embarass the worst historian. Seliger himself remarks that "an unhistorical attitude is at the root of much of the talk about Locke's inconsistency or ambiguity . . . the resulting contradictions or ambiguities are often of the interpreter's than of the philosopher's making." (p. 33) I agree. At least, applying that suicidal judgment to his own book, a bad affair is brought to an honorable conclusion. —RICHARD ASHCRAFT, *University of California, Los Angeles.*

Toward a Mathematics of Politics. BY GORDON TULLOCK (Ann Arbor: University of Michigan Press, 1967. Pp. 172. $7.95.)

Gordon Tullock writes of a diversity of topics: communications, lying, political persuasion, monopolistic profits, the aggregation of political and economic preferences, and proportional representation. These distinct subjects are linked in two ways. Throughout, man is assumed rational in the sense that he is engaged in maximizing behavior. Tullock is most concerned with specification of choices relevant to maximizing in various contexts. When it might "pay" to be politically ignorant, is an example of one context of interest. A second unifying characteristic of the book is that the discussion of almost all topics is accompanied by geometric illustrations and formulae of a sort. Still, though, we had best take Tullock at his word (p. iv) and not expect one theme to pervade the book.

A chapter of particular interest to many political scientists is that on the irrelevance of Arrow's impossibility theorem for large groups of individuals faced with a limited number of policy alternatives. As applied this theorem simply states that it *may* be impossible for a majority to agree on any proposal over all others, although majorities often form to favor one proposal over one other. Tullock's argument is based upon voting rules actually employed, particularly in legislative bodies. First, hair-splitting amendments are not allowed. Second, procedure usually consists of making paired comparisons among alternatives, but not all combinations are offered. Voting is not a ranking process *per se*. This, by itself, does not indicate the irrelevance of the theorem in a strict sense; institutional arrangements make it irrelevant, so long as any set of policies each of which commands majority support over any policy not in the set is reasonably compact. Once a point in this set is obtained the cost to an individual of attempting a change to another such point in the face of institutional and conventional obstacles is probably greater than benefits gained. Lack of endeavor to change policy, to retrace legislative action (p. 47), can not, then, be taken as evidence that circularities do not exist.

The consideration of costs missing in the discussion of the impossibility theorem is found in Tullock's discussion of monopolistic competition. Population density and the consumer's aversion to traveling to a good are at least two variables not usually found in the micro-economic or anti-trust literature. In this chapter and those on persuasion, information and lying, Tullock presents costs and calculations in an analytic style sure to reward political scientists interested in these topics. Additionally it is a joy to find someone who at least attempts to distinguish between hypotheses and guesses about fact, a distinction not made in most of our literature. For these reasons and because it is the most thoroughly argued chapter in the book, the discussion of monopolistic competition stands out.

There are hypotheses in this book. Some are confusing, some implicit, but that they are there at all is salutary. Confusion reigns in consideration of the direction of the effect of population density on store size, prices and profits in monopolistic competition. Density would increase store size and lower price, except that density creates congestion which raises transportation costs. Tullock really only indicates variables to be contained in hypotheses in this instance. However, in the chapters on information, propaganda and lying, there are several hypotheses on the comparative level that might easily be passed over (eg. pp. 86–87). These hypotheses have no numerical aspects.

Interestingly, none of the hypotheses in this book are mathematical; that is none involve rational functions of intervally measured variables. The formulae which occur are best taken to indicate the hypothesized direction and additive or multiplicative form of the relationship among variables, if intervally measure (eg. pp. 109, 124, 126, 137). Actual formulae employed are for illustrative purposes (eg. the equation and subsequent differentials on pp. 75–76). Sometimes only the fact that a hypothesis exists is indicated (eg. p. 94). Further, the geometrical illustrations are of arbitrary instances of the hypotheses or facts presented; no hypothesis is presented *per se* as a geometrical construction.

If functions of intervally measured properties are not found in this book, why the title? Part of the responsibility does not rest with Tullock. Among young or "current" political scientists mathematics is often equated with doing science. Nothing could be further from the truth. Mathematics is a tool in empirical research. Gimmicks such as "causal analysis" are misused tools. Certainly advanced sciences possess functional laws, but doesn't this indicate an appropriate emphasis on concept formation, on the definition of intervally measured properties so we can use the tool and not concentration on the gimmicks? Fortunately Tullock's text does not betray the misunderstanding of the title.

Tullock's introduction does betray such a misunderstanding. He talks in terms of a general (probably "mathematical") theory of political science and economics. Isn't this putting the cart before the horse at a time when one would be hard pressed to even state many testable (not vague) hypotheses? However, I will not take the introduction too seriously because the rest of the book does not depend upon it.

Tullock's first and last chapters are clearly the weakest. A principle reason for this is that he sets up straw men which are either oversimplified or nonexistent. When he argues his own brand of "utility theory" in chapter one, I simply do not recognize the extreme positions he argues against. Tullock's arguments against depreciators of the non-informative aspects of advertising are weak, because they also counter absurd positions. Much more subtle positions exist than those Tullock presents for the opposition. What about the arguments concerning the effects of advertising on expenditures on private versus those on public goods *in the long run?* What about long run effects on taste? However, Tullock's real problem is simply that the subjects of chapter one deserves a book. So does the last chapter. We know very much more about the effects of P.R. and plurality representation than Tullock presents; he argues against what must be considered even an over-simplification of Duverger. Tullock clearly prefers P.R. and, unfortunately, unlike any other discussion in the book, the preference distorts the picture. However, most of the rest of the book is very objective, very thoughtful, very clearly presented, and very enjoyable reading.—E. W. KELLEY, *University of Chicago.*

Middle Class Radicalism: The Social Bases of the British Campaign for Nuclear Disarmament. BY FRANK PARKIN. (New York: Frederick A. Praeger, Publishers, 1968. Pp. 207. $6.50.)

Students of mass movements will find this study of the social bases of the British Campaign for Nuclear Disarmament (CND) of great interest. Sociologist Frank Parkin has approached his task effectively, both empirically and intuitively. His analysis is based on a questionnaire drawn from some 445 CND followers, the literature produced in the course of the campaign, as well as on selected interviews. He treats the youth, Labour Party, church and intellectual components of the CND as well as the political impact on the Left, including the British Communists.

Parkin suggests that his findings question some well-known formulations relating to mass movements. He asserts that the thesis suggesting that mass movements derive a good part of their following from the dispossesed classes, the alienated, the atomized and the futile is not supported by his data. All but a small portion of his sample were of middle or upper class status, were involved in other organizations, and were optimistic both about the remediability of the human condition and their own efficacy in this quest.

As Parkin unfolds his evidence significant modifications in his own challenge to the traditional theses must be made. For example, in his last chapter he points out that large numbers of his sample, though "middle class" were only marginally so—that is, of other class origin and/or involved in the "independent" professions (university lecturers, physicians, clergymen, freelance intellectuals, etc.), while relatively few CNDers were from Establishment or "interdependent" professions (such as the civil service or commerce) giving strong sense of class and social identity. In terms of Lenski's hypothesis, the typical CND follower's different statuses (e.g., ethnic, occupational, economic, educational) were out of line with one another, in that he ranked high on some but low on others. In light of this conflicted model Parkin's earlier statistic indicating typical participation by CNDers in other organizations could well suggest the attempt of the relatively isolated and marginal to *overcome* that isolation as stated in the usual formulations of Arendt, Hoffer *et al.,* rather than indicating lack of isolation. This is especially indicated since the organizations to which CND followers tended to belong ("activist" and "moral orientation") would give them more identity with their isolated fellows than it would provide "intermediate structures" connecting them with the solid middle class or society as a whole.

This example only illuminates a central problem of which Parkin seems very aware. In what senses was the typical CND supporter "middle class?" Income, occupation, family and even education are indelicate tools for this task. Parkin himself sees the concept of "middle class" as mainly useful "in making contrasts with other social strata." Accordingly, the author attempts to analyze the differing characteristics of various groups within the CND, while maintaining the middle class description for the movement as a whole. Consequently, he fails to really exploit the theoretical implications of his own finding that his respondents overwhelmingly rejected prevailing middle class values: monarchy, nationalism, religion and capitalism. The great majority (58%) were non-believers—agnostics, atheists and so forth. Parkin also suggests that the CNDer, though typically "middle class," has "moral" rather than material interests to promote, unlike lower class mass movements. This is the heart of the problem. If the CND follower substantially rejects both the material interests and attitudes of the middle class, is conflicted in his middle class attributes and is not a member of the middle class as a social or political actor, then of what use is this very broad category except to register what become really secondary and only loosely functioning characteristics? The problem is more than semantic because Parkin never sufficiently frees himself from his middle class focus to develop the theoretical implications of his own data. In short, Parkin is not really discussing middle class radicalism at all but *intellectual* radicalism.

It was the intellectual component of the CND that gave it its distinctive ethos; indeed, as Parkin points out, without the intellectuals it is doubtful that the CND would have been created at all. If either the terms "intellectuals" or "middle class"' is to have any real meaning for the analysis of modern society they should be defined and treated separately. For most purposes, "middle class intellectual" is a contradiction in terms. In contemporary, mass-literacy societies where large portions of the population may be engaged in the manipulation of the symbolic environment as a vocation the term "intellectual" is very loosely applied. Parkin must have such a large and indistinct group in mind when he says that "political conformity, in the broad sense, is a more typical position of this stratum" than dissent. A far more useful approach would have been to sharply delineate the qualities of mind attendant upon that relatively small group of creative intellectuals which dissent and criticize in every society, albeit to different degrees depending on the conditions of political and social power. It was to this component that Parkin himself was pointing when revealing the remarkable consistency of many of his respondents in their socialism, internationalism and pacifism—in short, all those "apolitical" traits of that stratum which derives its sense of being and action from a shared world-view rooted in various degrees of alienation. Once one had detected this intellectual core for the Campaign for Nuclear Disarmament by giving the intellectual more discrete treatment the author's extensive evidence that the CND was an umbrella covering a wide range of anti-Establishment (i.e. anti-middle class) grievances becomes more meaningful. Parkin could have very usefully dispensed with the notion of middle class and treated the intellectual stratum which provided the core of the CND as a separate phenomenon.—G. ERIC HANSEN, *Massachusetts Institute of Technology.*

The Origins of American Politics. BY BERNARD BAILYN. (New York: Alfred A. Knopf, 1968. Pp. 161. $4.95.)

Liberty and Authority: Early American Political Ideology, 1689–1763. BY LAWRENCE H. LEDER. (Chicago: Quadrangle Books, 1968. Pp. 146. $5.50.)

These two volumes by historians shed considerable light on American political thought prior to the Revolution. Both books deal extensively with the concepts of power and liberty in the colonies during the eighteenth century and help to fill a gap in the literature.

In his work which is an expansion of the Culver Lectures at Brown University in 1965, Bernard Bailyn, presently Winthrop Professor of History at Harvard, examines the differences between the political processes in eighteenth-century America and eighteenth-century England that can aid in explaining "the significantly different receptions of the same political ideas." While noting the common British culture and belief in certain Lockean political principles in England and the American colonies, the author emphasizes the differences in the role of the executive in the two areas. Except for Rhode Island and Connecticut, the executive power was legally "far stronger" in the colonies than in the Mother Country. However, the actual power of the colonial governors was greatly reduced because of rigid instructions and an increasing concentration of patronage powers in England. Thus, the governors were denied important weapons which were available to the Crown and its ministers in asserting influence and stifling dissent in Parliament.

The last chapter of Bailyn's volume is devoted to a number of examples of how the colonial governors actually exercised little political influence despite their legally autocratic power. Although concentrating on the situations in the royal colonies, the author claims that the pattern was not substantially different in the proprietary colonies, for the "Quaker party" exercised strong control over the Pennsylvania House in the 1740's and '50's and the Maryland House of Delegates reacted vigorously to the use of strong proprietary power. Bailyn states that democracy in many ways was more advanced in the colonies than in England; thus, there was greater opportunity for radical opposition to overweaning power and authority to be expressed and organized. Playing down the importance of the Stamp Act as an incendiary action in itself, the author claims that there was already a colonial mind that was steeped in an ideology which emphasized the evil of despotic power; therefore, the actions of Parliament in the 1760's and 1770's were more suspect than they otherwise might have been.

Although Bailyn's argument and documentation are very convincing, he might have devoted more space to the lack of an American aristocracy in explaining the differences in the influence of the English and colonial radicals in the eighteenth century. The Tocquevillian insight, which has been developed so well by Louis Hartz, that Americans were "born equal" should certainly be taken into account in an examination of the power structure in the colonial period immediately prior to the Revolution.

Lawrence Leder, presently Professor of History at Louisiana State University in New Orleans, gives credit to Bernard Bailyn for producing the "one major effort to penetrate the ideas held by Americans in general" in the eighteenth century and then proceeds to examine the ideological conflict between liberty and authority in the colonies in that period. The development of freedom of expression and especially the role of the press as a critic of the arbitrary use of power are noted as important factors in the revolutionary movement. The author shares the conventional view of the importance of Lockean principles in mid-eighteenth century America, including religious toleration.

Leder states that a crucial consideration in the political discussions in the colonies was the actual nature of the British constitution. He notes that two general approaches are found in eighteenth-century American writings on the subject—that the British constitution was fixed and rigid and that Parliament was supreme and could change the constitution. Although a couple of citations from colonial periodicals are used to indicate support for the position of Parliamentary supremacy, there is not much evidence to show that the colonists generally understood the implications of the Glorious Revolution. The author does state that from 1749 on, the "myth" of a fixed British constitution was not effectively challenged in colonial writings.

Leder devotes a chapter to the debates over the nature of colonial charters. He notes that the disputations often arose in connection with the respective powers of the executive and the popularly-elected legislature and very frequently concerned money matters. The disputes over powers in the royal and proprietary colonies were never settled, whereas the debates over prerogatives in the charter colonies did lead to "an increasingly well-defined concept of charter rights." The disagreement among the colonies on the interpretation of the colonial charters did not allow for a united front against the British on the grounds of local constitutional principles.

In the reviewer's opinion, Leder's most effective chapter discusses the importance of rights and liberties in the pre-Revolutionary argument. Noting that the colonists developed a "simplistic version" of the Magna Carta and the rights of Englishmen, the author points out that these eighteenth-century Americans did not define their rights or liberties except in broad generalities as a rule. Here Leder ties in with Daniel Boorstin's theory of the "giveness" of American political principles which are only vaguely defined if defined at all. The author points out that this broad concept of rights often forced the British to formulate a definition of the principle in question. However, the Lockean rights were absolute and thus the colonists could respond that any limitation on these liberties was an example of the improper use of power.

These small volumes should prove valuable to students of American political thought and colonial government. Of the two, the Bailyn book makes more of an original contribution, but Leder's study also contains an interesting analysis of American political ideology prior to 1763.—DON W. DRIGGS, *University of Nevada*

Information Retrieval: Applications to Political Science. BY KENNETH JANDA. Indianapolis: Bobbs-Merrill Co., Inc. 1968. Pp. 230. $4.50.)

Information Retrieval is primarily a collection of articles which the author has published previously. The volume is organized into three sections which emphasize, repsectively, retrieval techniques, their application to political research, and their application to administrative and legislative decision making.

As Professor Janda demonstrates in this work, the potential contribution of modern information processing technology to the science of politics includes a great deal more than the computerized analysis of quantitative data. In Janda's words, "techniques of information retrieval promise to assume an important place in the activities and concerns of political science, for they offer the political scientist new methods of handling the vast flood of written materials that constitute both the data and the findings of political research."

Of course, the application of computers to non-numerical materials is by no means a new endeavor as the growing literature on content analysis will attest. What is significant about the studies described by Janda, and by most work on the automated retrieval of politically relevant information, is not simply that textual data can be treated, but that because it can, the value of retrieval systems to the political scientist can be realized during the earliest stages of research. For by making possible wider and more rapid access to original documentary data and to the published accounts of other scientists, information retrieval systems are able to contribute to the critical processes of problem conceptualization and the formulation of hypotheses.

Janda's discussion of the formidable intellectual problems associated with classifying the contents of a document so that it can be obtained by all retrieval system users interested in it is first rate. Illustrating how 'key word' methods have been employed, he describes the preparation of the *Cumulative Index to the APSR* (1906–1963) and how KWIC and KWOC programs were used to search through abstracts of literature in order to identify data sources for the International Study of Values Project.

Among the retrieval applications which Janda covers are: Northwestern University's system for automatically disseminating articles drawn from leading social science journals to faculty members who have previously designated their areas of interest, a hybrid computer-microfilm system permitting rapid and selective access to recent literature on political parties, and computer generated key word indices to roll call votes in the Kansas Senate, the U. S. House of Representatives, and the U. N. General Assembly.

The final chapters of *Information Retrieval* set forth a group of proposals; the most interesting of these is for providing extensive improvements in the process whereby information is handled by the Congress. Janda analyzes the kinds of information services that the Congress, its chambers, its committees, and individual Congressmen might wish to utilize. Basically, he argues that the oft-alleged decrease of congressional *vis-a-vis* executive power can be most effectively redressed not by reorganization or political reform, but by 'retooling' the Congress with new information systems.

By choosing to focus upon a limited number of information retrieval projects, Janda has created a work that I find to be admirably specific and remarkable for the conceptual integrity of its contents. It conveys an awareness of the major theoretical and operational problems in this field by confronting the reader with the details of their resolution in each of the projects examined. Throughout the volume, Janda's discussions reveal a balanced sensitivity to the practical as well as the conceptual aspects of retrieval. However, the principal emphasis is always directed to those issues which most greatly affect the development of improved ways of securing for the political scientist rapid access to the information he seeks.

Janda's writing is lucid and his manner of presentation forthright. Nonetheless, this is not an easy book to read. Its subject matter is technical, and although the casual reader will acquire some understanding of the kinds of functions that information retrieval systems can perform, a full appreciation of the material covered wlll be accessible only to those who are either already familiar with retrieval techniques, or to those willing to invest the effort required for a close reading of this work. This caveat, however, sould not deter the uninitiated. On the contrary, Janda's book is an excellent starting point from which to learn the fundamentals of information retrieval. Indeed, when viewed as an introductory volume, even the defect of redundancy among portions of several of the articles emerges as a virtue, permitting the reader to review the details of a given technique and to understand its use in a variety of applications.

To summarize, Professor Janda's *Information Retrieval* is highly instructive, and, in my opinion, represents a valuable contribution to the discipline.—EDWARD FRIEDLAND, *State University of New York at Stony Brook.*

Social Theory and African Tribal Organization: The Development of Socio-Legal Theory. BY KENNETH S. CARLSTON. (Urbana: University of Illinois Press, 1968. Pp. 462. $10.00.)

The dictum that form follows function applies as much to works of scholarship as to skyscrapers or armchairs. Thus the only fundamental difficulty with Kenneth Carlston's most recent book is a question of manner rather than matter. This is particularly unfortunate, for in many respects this work contains much of great interest and importance.

The bulk of the book is given over to brief descriptions of twenty tribal societies, drawn from East and West Africa. Despite their brevity (seven appear merely as abstracts of the literature), an immense quantity of the ethnographic literature appears here systematically and lucidly presented. Of necessity, many of the tribes Carlston has chosen are those most thoroughly studied, such as the Tiv and the Nuer. In any case, these miniature portraits show once again that we know more (or at least can *say* more) about societies culturally and physically remote from us than about those in which we normally function and which occupy the major energies of the social sciences. In reading these chapters we can only wish someday to see comparable ethnographies of American society and of international relations.

Carlston has chosen quite properly to emphasize some elements of the ethnographic data to the exclusion of others, notably the rich material on order and conflict. The works he draws upon, by British social anthropologists and their students, are peculiarly laden with such data. Why this should be so is itself an intriguing question in the sociology of knowledge.

But unlike other who have written on law in tribal societies, such as E. Adamson Hoebel, Carlston gives substantial weight to the role of religion and the supernatural as part of the sanctioning process. This continual counterpoint of religion and social conflict has three major implications: First, we often arbitrarily divide "reality" into tangible and intangible parts; by concentrating on the former alone we miss the vital connection between beliefs and actions. Second, if religion is to be considered part of the sanctioning process, legal theorists must drastically overhaul their concept of the nature of sanctions, and we might well ask whether the term should not be dropped entirely. Third, Carlston implies that there are levels of both order and conflict, and that law regarding each should be separately considered. To this end, he places great stress on a distinction between conflicts within an accepted social order and "constitutive conflict," which challenges the validity of that order. This in turn suggests that law is most accurately pictured as a multiple, "layered" phenomenon, ranging from the most basic social values (close to Hoebel's "jural postulates") to subordinate, related values, disputes about which need not produce major fissures in the society.

If there is a major flaw in the work, it lies in its peculiar architecture, for the ethnographic material is sandwiched between two large sets of propositions. The first is a deductively arranged summary of the social theory that may be found in Carlston's earlier books, *Law and the Structures of Social Action* (1956) and *Law and Organization in World Society* (1962). While this is useful to have in itself it is intended to somehow link up with the second set of propositions at the conclusion of the volume. These, however, have been inductively generated out of the ethnographies. If they are meant to serve as a test of the first set of statements, this has not been made clear. The first set may not in fact consist wholly of testable statements, and in any case the subheadings of the two chapters are not always parallel. Carlston is at pains to point out that the concluding observations are valid only insofar as they apply to the tribes he has studied and to emphasize this has appended after each proposition the numbers of the chapters that furnished the evidence. But it is difficult to know how to make use of any two statements, one of which may be true of twenty tribes, the other of only one. Yet the broadly general and the uniquely particular coexist within the same purportedly logical framework.—MICHAEL BARKUN, *Syracuse University.*

The First Bolshevik: A Political Biography of Peter Tkachev. BY ALBERT L. WEEKS. (New York: New York University Press, 1968. Pp. 221. $7.50.)

Scholarly eyebrows will no doubt rise in response to Professor Weeks' seemingly presumptuous contention that an oft-mentioned but rarely studied nineteenth century Russian Jacobin socialist, Peter Tkachev, was, in reality, "the First Bolshevik." Whatever the reaction, the man Lenin called "the magnificent Tkachev" has been a victim of neglect. Albert Weeks has attempted to rectify this by locating the roots of Tkachev's philosophy within the tradition of nineteenth century Russian radicalism and by arguing that Tkachev's writings influenced V. I. Lenin who, if not the first, remains the most important Bolshevik.

Born of "landowning peasants of modest means," Tkachev drifted toward radicalism while a university student. Subsequently arrested, tried and exiled for his conspiratorial activities, he escaped to Switzerland and joined the circle of radical Russian emigrees. Until his death in 1886, at the age of forty-one, Tkachev published the revolutionary journal *Nabat* (*Alarm*) and participated in the activities of secret societies dedicated to the destruction of the Tsarist regime. Professor Weeks contends that Tkachev's ideology was a form of Jacobin radicalism which contributed to the philosophical transition from Populism to Bolshevism among Russia's modernizing intellectuals in the final quarter of the century. Therefore, he has devoted the major portion of his book to clarifying the principles and identifying the problems of Tkachevism.

Weeks focuses first upon Herzen's Populist movement of the 1840's which eventually yielded to the more extreme *raznochinstvo* philosophy of men like Belinsky, Chernyshevsky, Dobrolyubov and Pisarev. He maintains that Tkachev followed in this tradition and, in doing so, made certain essential contributions to Bolshevism. The most important of these were his concept of party organization and his belief that Russia was a special revolutionary case.

Tkachev had no faith in the masses as a revolutionary force. He believed that a strictly organized party of intellectuals, oriented toward the seizure of political power, was an absolute prerequisite to a successful revolution. Tkachev recognized the importance of propaganda and agitation in the post-revolutionary stage, and he envisioned a party dictatorship with no limits upon the means which it could employ to impose the new socialist order. Contrary to the strict Marxist revolutionary formula, Tkachev contended that revolution was possible in Russia because it was the "weak link" in the chain of capitalist nations. The source of Russia's uniqueness was, in Tkachev's mind, her instinctively and traditionally communistic peasantry.

The thesis that Tkachev was the "first Bolshevik" is more difficult to defend, however, with respect to other important aspects of his thought. The principles of Tkachev's economics, for example, are confusing. He wrote, ". . . all phenomena of a political, moral or intellectual nature . . . are the result of causal phenomena in the economic sphere" (p. 134). Notwithstanding such strains of Marxism, Tkachev's economic outlook was much closer to classical utilitarianism than it was to economic determinism, and Professor Weeks writes that, "Tkachev places the strongest emphasis on . . . the commanding position of politics over economics, over society . . ." (p. 112). Additional departures from Bolshevism's Marxist heritage which deserve mention include Tkachev's rejection of the notion of historical materialism and his low level of class consciousness manifested in his belief that the state could function above the interests of any particular class.

But doubt concerning Tkachev's adherence to Marxist principles would be secondary if his philosophical influence upon Lenin could be firmly established. With the exception of the title of his book, Professor Weeks is rather conservative in his conclusions on this point. There is simply no concrete evidence to prove that Lenin consciously or unconsciously drew upon Tkachev's writings for his ideas and, as Weeks notes, "to gauge the influence on one man of the reading of another's books is a most delicate task" (p. 74). Having admitted this, Weeks can only make the conditional statement that, "in the writings of Tkachev there may be found certain significant concepts which may be described as proto-Bolshevik" (p. 74). The task of measuring Tkachev's contribution to the development of Bolshevism has been further frustrated by the refusal of Lenin's Soviet heirs to recognize any ideological connection with Tkachevism. In his efforts to surmount these difficulties, Professor Weeks has overemphasized the existing similarities between the ideas of Tkachev and Lenin, thereby jeopardizing the balance of an otherwise thoughtful analysis.

Even so, this study of Tkachev remains an extremely cogent contribution to the literature on Bolshevik history. Professor Weeks' heavy emphasis upon Tkachev's own words, most of which he has translated for the first time, along with the complete listing of Tkachev's works which he has appended are sufficient justification for considering this an important volume. The author's insightful analysis of the subject makes it essential reading for all students of Communism.—PETER R. ZWICK, *Louisiana State University.*

The Emergence of the Eastern World: Seven Essays on Political Ideas. BY G. L. SEIDLER. (Oxford: Pergamon Press, 1968. Pp. 260. $10.00.)

Grzegorz Leopold Seidler is Rector of Marie Curie Sklodowska University at Lublin, Poland. His book is an attempt to remedy the neglect of Eastern political ideas that has long characterized the study of political thought in Western Europe and the United States. To that end, Seidler has written seven essays dealing with various aspects of Eastern political thought in the period between 3000 B.C. (in the case of Egypt and India) and the sixteenth century A.D.

Rather than undertaking a philosophical analysis of certain selected ideas, the author uses a genetic approach—political ideas are placed in a general socio-economic context and the origins and historical impact of the ideas are traced. In this respect, what Seidler says about one of his essays might well be applied to the whole book, namely, that it "will not bring to light any new facts, nor will it attempt to throw new light on the facts already known" (p. 227). When one considers the relative lack of familiarity most readers will have with the contents of this work, the kind of historical discussion it offers is probably what is initially needed to focus further attention on Eastern political thought.

The topics considered in the individual chapters extend from the very broad matter of ancient Eastern political ideas to the much narrower subject of the Jagiellonian monarchy in Poland. The first essay discusses the beginnings of Eastern political thought (and, indeed, of political thought itself) in ancient China, India, Babylon, Israel, and Egypt. Here emphasis is placed on the religious sources of the political doctrines of the ancient Eastern states, with most attention given to China and India. The period between the fall of Rome and the Hundred Year's War is represented by three essays dealing with "Islam as a Political Doctrine," "Byzantine Political Thought," and "The Political Doctrine of the Mongols." In the fifth chapter, "Political Thought in the Period of Transition (1350–1450)," the decline of European universalism provides the theme for discussions of the Conciliar Movement and the rise of religio-national opposition stemming from John Wyclif in England and John Hus in Bohemia; the collapse of the Islamic universal Empire sets the stage for a portrayal of Ibn Khaldun as the proponent of a new political realism. Two more specialized essays conclude the book: "The Muscovite Autocracy" and "The Jagiellonion Dynasty."

To take the book as a whole, certain peculiarities of interpretation stand out in what is for the most part an objective, if pedestrian, account of Eastern political ideas. Throughout the work, Seidler places special emphasis on class conflict and on the class character of political ideas. The author often lapses into what appear to be Marxian phraseology and formulae in explaining political ideas in terms of the correspondence or non-correspondence of social relations and structural forms to economic factors. This same line of thought is also suggested by the author's response to Sir Ernest Barker's observation that Byzantine political thought displays little originality or creativity. Seidler comments: "The value of political doctrines does not depend on the degree of their originality but on the social function they fulfilled or fulfil. The strength and importance of a given political idea is decided first of all by the degree in which it expresses class interests and by its effect on social relations and legal institutions." (p. 140).

Given this interpretive bias, which stresses "objective material factors" in understanding the origin and significance of political ideas, it is not surprising that the author also eschews what he calls subjective or idealistic interpretations of politico-religious ideas. In the case of Mohammed, for instance, Seidler dismisses as "typically idealistic" (p. 90) Tor Andrae's thesis that the personality of the Prophet was the dominant factor in the rise of Islam and asserts that Mohammed, though unaware of it himself, "was the man in whom the objective development of Arabia was mirrored" (p. 69). In a similar vein is the comment that the visions experienced by Mohammed at the outset of his career as religious leader were the product of "his nervous and sickly imagination" (p. 61). In sum, the various essays exhibit a tendency to deny any significance to politico-religious ideas and doctrines aside from the extent to which they manifest class interests or economic forces and serve as ideological weapons in the class struggle.

G. L. Seidler has written other works on jurisprudence and the history of political thought, but this is his first book to appear in English. Frankly, it is not well-written. There are numerous failures of style and syntax. The essays are developed in disjointed fashion and tend to be repetitive.

Despite its defects, however, this book does help to fill the gap in our knowledge of Eastern political thought. The work is noteworthy because it brings together under one cover brief surveys of the main schools of Eastern political ideas. The essays, particularly those on Byzantine thought, the Muscovite autocracy, and the Jagiellonian dynasty, draw heavily on Eastern European and Russian sources which may be unknown or less accessible to students of Eastern political thought in the United States.—WILLIAM J. KIRSCH, *Arizona State University*.

AMERICAN GOVERNMENT AND POLITICS

Democracy and the Public Service. BY FREDERICK C. MOSHER (New York: Oxford University Press, 1968. Pp. 219, $4.75.)

Reviewing one of the numerous Brookings Institution studies of our public bureaucrats a few years ago, the author of the book presently under review said: "One concludes this work like other studies of this kind, with a sense of vague disquiet: that he has added significantly to his knowledge; but that he needs a conceptual framework about American society and its government within which, or against which, to fit the empirical findings." (This REVIEW, September, 1966, p. 700.)

One suspects that this was the goal of Mosher's present study, the first in a new series on Public Administration and Democracy under the general editorship of Roscoe C. Martin of Syracuse. And it must be said that the author brings to his quest a distinguished reputation in the field, both as an extensively published scholar and as a past holder of several national administration positions. On the record, then, Frederick Mosher merits an immediate audience when he speaks to the subject of democracy and the public service.

Eschewing at the outset an extensive definitional analysis of the meanings of democracy, Mosher rather constructs a syndrome of social and political characteristics, the nature of which, taken together, is designed to give us a reading on the democratic health of the public service. Most of these characteristics are familiar: Responsibility, Representativeness, Mobility, Participation, Non-Elitism, and the Rights of Public Servants. In summary fashion, the author proceeds to comment on each. "Responsibility" is ambiguous as to authority (legal and political accountability vs. subjective allegiances). "Representativeness" is complex (active vs. passive; legal vs. political interest). "Mobility" is misleading (the classical rise in class vs. horizontal-field, public-private, national-state-local, or other possible planes of movement). "Participation" is popular today, but dilemma-ridden as it applies to administration (the imperatives of rational, institutional logic vs. pluralistic, democratic involvement). "Elitism" is *a priori* undemocratic. (A single, cohesive national elite is intolerable in theory and unverifiable in practice, but many specialized sub-elities appear active. Mills and Hunter went too far.) Finally, public servants should have citizens' rights, but these are infringed in many ways (restrictions imposed by loyalty-security programs, narrowly limited rights of collective bargaining, and so on.)

These then are the major problem areas. The remainder of the book is devoted largely to a description of the appointive public bureaucracy in the United States, relating characteristics of the service to the categories of concern for democracy. Mosher makes ample use of recent research literature on bureaucracy dealing with social backgrounds, social characteristics, image, etc. In addition, he supplies personal evaluation and reasoned assumptions based upon his own experience. About one-third of the book is devoted to the history of the relationship between the public service and education and to the evolution of civil service concepts from 1789 to the present ("Government by Gentlemen" to "Government by Administrators").

While what has gone before its informative (the degree depending, of course, on one's background) it is at page 99, when the author embarks on an analysis of "The Professional State" (Chapter IV), that interest really livens. At this point we are led through an examination of that characteristic of the higher bureaucracy that has most recently emerged and that is most portentous both for the bureaucracy itself and for democracy. In numbers, over one-third of all (U.S. Census defined) professionals are employed by American governments and these governments employ more than three times as many professionals as private organizations. Indeed government even stimulates the creation of new ("emergent") professions—some of which have little or no employment market but government, e.g.—penologists and air pollution specialists.

This phenomenon raises a host of questions about its impact and only tentative answers can be offered while many intriguing hypotheses are prompted. The prodigious specialization that one finds in the public service—specialized expertise based upon narrow and deep training—has resulted in a proliferation of varying experiential backgrounds, approaches, methodologies, mentalities, self-perceptions, and desires. Given the proportionate size and pluralistic make-up of professionals in the public service, what use can one expect to make of traditional ideas about hierarchy, authority, efficiency, merit and conflict in administration? Perhaps something like the collegiality of professors has largely replaced hierarchical authority among professionals in government. Perhaps the professionalization of the public service throws new light on the old Friedrich-Finer debate about the nature of administrative responsibility, light incidentally, that seems to support Friedrich's side if we assume a more pervasive commitment to one's professional orientation. Perhaps, as Mosher's modestly stated "unproved observation" suggests, key conflict areas are those between different professions or segments of professions in different agencies, rather than between management and the workers.

In thinking about the future, Mosher in the end offers a key: higher education in America. Thus, at bottom, is is implied (and this reviewer agrees) that if we are to shape the future of the public service along democratic lines, our options are probably more dependent upon what comes to pass on the campuses of the nation than on institutional modifications or "reforms" within the bureaucracy. There are very real congruencies between what Jencks and Riesman are talking about in *The Academic Revolution* and what Mosher considers here. It may not be a novel idea that the future of the public bureaucracy is inextricably tied up with the ongoing organizational revolution in our society, but it is an idea which finds additional evidence and dimension in this book.

In sum, *Democracy and the Public Service* is a useful and informative synthesis of what we need to know about the public service now and to some extent, a suggestive one about where we might forage for intelligence concerning the future of the relationship considered. In fairness, it must be said that what we do not have here is a rigorous and comprehensive new model blending theory and data, but one could have a far worse organizing principle than the qualities of democracy for presenting facts about and trends in American bureaucracy. For students (especially since it is available in paperback edition) of administration and government, it offers much more of interest and importance than the traditional three or four textbook chapters on personnel administration.—CHARLES E. JACOB, *Rutgers University.*

The Information Machine: The United States Information Agency and American Foreign Policy. BY ROBERT E. ELDER. (Syracuse, New York: Syracuse University Press, 1968. Pp. 356. $10.00).

If the title and Table of Contents of a book faithfully predicted what its covers contained, this study of the United States Information Agency should be one of compelling interest to students of American foreign policy and administrative decision-making. In his Preface, professor Elder promises to reveal "how USIA presently functions, how it receives policy guidance or influences the formulation of foreign policy, how it translates foreign policy into information policy, and how it transforms policy into programs and products." His analysis, he announces and further whets the appetite of a potential reader, will focus on a complex web of "inter- and intra-agency contacts, of lateral and vertical relationships, with emphasis upon interactions affecting policy, program, or product." To a considerable extent, the author delivers on his promise. At great length and at times in exasperating detail Professor Elder describes the evolution of USIA, its changing missions and its somewhat awkward role in the confusing network of foreign policy-making and maps the various streams of decision and action within that network through which the agency attempts (a) to explain America to the world, (b) to support positively and pointedly America's foreign policy objectives, and (c) to establish credibility for both to a skeptical global audience. This is an ambitious assignment for USIA and if Professor Elder's study does not completely meet the requirements he set for himself, the explanation may lie as much in the agency's gropings as in the author's conceptual and methodological problems.

An information function serving foreign policy needs existed off and on long before the United States Agency was created in 1953, although it tended by be "psychwar" in purpose and was designed to meet the strategic needs of military operations. It was not until 1948, and then largely in response to intensifying Soviet propaganda efforts in Europe and the crystallization of the Cold War, that the Congress specifically authorized within the Department of State a "worldwide information and cultural exchange program." Therefore, when an independent information agency was created by President Eisenhower's Reorganization Plan No. 8 in 1953, the basic components of what became USIA were already in operation. Several of those components—the Voice of America, the USIS libraries, the binational centers, the Russian language periodical *America Illustrated*—became familiar to Americans at home even though none of USIA's activities are directed to or consumed by the domestic market.

Professor Elder's emphasis is on USIA as a complex, flexible, far-flung organization, responding over the years to an array of influences and changes in mission. His analytical framework develops around the agency's structure (the Office of the Director, the Office of Policy and Research, the Area Offices, and the Media Services) and its functional responsibilities. The evolution of each organizational and functional component is described, frequently at some length, and each component's relationship internally to other USIA units and functions and externally to counterpart or complementary units and functions elsewhere in the Federal government, particularly in State and the Executive Office of the President, is mapped. Of considerable interest is Elder's rather skillful presentation of the impact of the organization environment—policy values, leadership styles, technological change both in a mechanical (advances in communications technology) and a cultural sense (new kinds of professionalism and expertise flowing from these advances)—on agency operations. His frequent discursive and detailed explorations of intra-agency relationships are both frustrating and tantalizing; it is difficult to decide whether they are an attempt to capture the "reality" of USIA existence through the "typical day in the life of an administrator" technique or an effort to *narrate* functional flow diagrams with words rather than through graphic presentations. The net effect, however tiresome the detours may seem at times, is a multi-dimensional grasp of the total organization.

USIA has had a turbulent, brief existence,one characterized by dramatic shifts in mission and alterations in organizational behavior. As Cold War tensions eased in mid-'fifties, the strident anticommunist propaganda themes that permeated agency operations gave way to softer tones designed to establish and broden USIA credibility with its overseas audiences. The late 'fifties found the agency primarily concerned with presenting the "full and fair picture" of America. The policy values guiding program emphasis shifted dramatically with the advent of the Kennedy Administration as programming increasingly was directed to selected audiences and designed "to achieve maximum influence" in support of specific foreign policy objectives. Under the Johnson Administration the "politicization of information policy" has been balanced by a renewed concern for cultural affairs and a new effort to tailor "media products" to suit individual country or regional requirements.

Each new turn in agency mission brought with it organizational adjustments that Professor Elder describes and analyzes in fine detail. The shifting functional roles in country planning, media services, and research and the fluid headquarters-field and USIA-State-White House relationships are exhaustively and at times exhaustingly documented. Since the book traces these adjustments and shifts for each organizational and functional component the reader soon is aware of a growing narrative redundancy which, given the conceptual and methodological structure used by the author, is perhaps unavoidable. Thus, the potential that PPBS has for USIA effectiveness is chewed over at least a dozen times, and the tensions between the "civil service" media specialists and the Foreign Service Career Reservists are analysed at least as many times.

Despite these caveats, I felt the volume a worthy investment of time for serious students of administrative behavior and American foreign policy. It treats with skill the persistent and still unresolved difficulty that any information-propaganda-cultural exchange function encounters in serving changing policy objectives which are pursued by a government committed, at least on the home front, to a free press and an open society. The continuous efforts to assure that policy shifts and shadings are accurately and functionally reflected in media output, particularly the so-called "fast media" of press and radio services, echo themes familiar to students of administrative planning and control. Finally, USIA's efforts to implement PPB through its comprehensive country programming system also strike a current note and reveal in a way that Elder is perhaps not aware of a major defect in the Federal PPB effort that purports to plan and program within departmental and agency program structures without the necessary "cross-walks" for inter-agency and inter-program coordination and decision that a government-wide program structure would provide. Without some such "common denominator" for program analysis USIA's efforts to work effectively with State, AID, and other members of the foreign policy team will be fraught with frustration.

This comprehensive study of USIA and the information function it contributes to the making and implementing of American foreign policy could have been a much more tightly organized volume. Both Professor Elder's study and USIA itself would benefit by a more deliberate cultivation of the applications of general systems theory and, perhaps symbolically, information theory. In time PPB may so inform the nation's "information machine." In the meantime, USIA will probably remain in a state of role uncertainty and mission ambiguity.—William R. Monat, *The Pennsylvania State University.*

Progressive Politics and Conservation: The Ballinger-Pinchot Affair. By James Penick, Jr. (Chicago, Illinois: University of Chicago Press, 1968. Pp. 207. $7.50.)

Penick applies techniques of manuscript analysis to provide additional understanding of one of the most famous fights in the administration of American natural resources. Drawing upon the Balinger, Pinchot, and Taft papers as well as the collections of several lesser figures of the time, Penick has identified and tried to set in fair perspective many facets of the controversy. The personalities, and the ideological conceptions, of the chief antagonists are woven into his story along with the perceptions the men had of the era, the issues, each other, and the various actions each took. A balanced presentation, from the standpoint of the warring factions, Penick's analysis neither vindicates Ballinger nor champions Pinchot, but rather sees each as acting righteously within his own convictions and within his conception of the public interest. Such judgment as he does levy, Penick reserves for Taft and Ballinger's mishandling of their side of the story, especially during the Congressional investigation of 1910.

Penick also tries to interpret the broader implications of the conservation movement and the meaning of the controversy in his opening and closing chapters. Both chapters, however, depend heavily upon the personal attitudes reflected in the papers the author reviewed and miss the type of insightful perspectives about the principal—and conflicting—elements within the conservation movement which Henry P. Caulfield, Jr. noted a decade ago. Both the preservationist and regulationist "thrusts" are too narrowly conceived, with, for example, the public power movement being entirely ignored. Thus Penick's conservation analysis centers mainly upon selected conflicts among development elements of the movement, chiefly those affecting the Land Office, the Forest Service, and the Reclamation Service. This focus leads him to conclude that the lesser issues ultimately revolved around the question of whether there was to be overall resource planning, and thus centralized management, or whether the various interests which wished to exploit public resources would be encouraged to continue their efforts. Ballinger, in these terms, is viewed as fighting centralized "bureaucracy" and applying business efficiences to resource management while Pinchot becomes the champion of scientific planning for resource management and thus of centralized, professionalized administration by qualified "non-political" specialists.

Although it contains many useful insights, Penick's book seems sure to win but few readers and to have but little application. An author's first obligation to his readers is to tell them clearly what he is writing about and to provide enough essential information so the reader can understand the issues and the action without having to conduct separate historical research. Penick fails this essential obligation. He assumes instead that all his readers will have a detailed knowledge of the issues and events which, in fact, but a handful of specialists possess. Thus he leaves out most of the key facts essential to understanding his story.

To compound his central failure, Penick never provides a consistent framework for his story. Instead he skips the reader back and forth from broad social concepts to individual personalities to public policy issues and then to the personal reactions of the participants in the sequence of bureaucratic infighting. Were the tale even chronological, the reader would have a fighting chance to follow him. But Penick prefers, as though he were adding a series of isolated historical footnotes to a story already told, to skip from topic to topic and back and forth in time without any seeming consistency. Thus his book degenerates into a series of enlightening footnotes about the reactions of individual participants to key events which he never presents or explains. For example, in the chapter on the Glavis charges, one never finds out what Glavis charged and in the chapter on the Congressional Investigation, one never learns what the investigating committee did or what it found!

Compounding this confusion is his extraordinarily difficult style of paragraph and sentence construction. He changes topics not only within paragraphs but within sentences as well and rarely bothers to open a paragraph with a topic sentence to aid the reader on his way.

Perhaps, however, public servants, politicians, professional conservationists, the interested public, and other scholars should not be too hasty to pass by such a work, even when its form of presentation submerges its valuable insights. The nation sorely needs more historians and other social scientists who can help us understand and interpret the issues and public policies which have affected our natural resources. Another recruit to the thin line of interested scholars is most welcome.—Daniel M. Ogden, Jr., *U.S. Department of the Interior.*

Federalism in the Southern Confederacy. By Curtis Arthur Amlund. (Washington, D.C.: Public Affairs Press, 1966. Pp. 140. $4.50.)

With all the attention Americans have devoted to the Civil War in the past century, it is surprising how few political scientists have studied it as a phenomenon of politcal behavior. In fact, between the time of Burgess and Wilson, when studies of the constitutional issues surrounding secession and the rebellion itself were fashionable in the discipline, and our own the lack of interest in the War can be seen as a bench mark of political scientists' general lack of interest in the behavioral aspects of American political history. Recently we have seen signs of a change in this attitude and we are beginning to apply questions, methods and techniques developed for the study of contemporary political behavior to the past with increasing success.

Professor Amlund's small volume, apparently the revision of his doctoral dissertation at the University of Minnesota, represents a modest and useful effort to raise the kinds of questions political scientists have asked about intergovernmental relations in recent decades and apply them to the abortive Confederate States of America. He is particularly interested in the relationship between constitutional theory and practice within the Confederacy in light of the exigencies of war. He concludes that, despite nuances of constitutional differences, in some cases written into the fundamental law of the C.S.A., there was essentially little difference between federalism in the Southern republic and that of the old Union which the Southern states were trying to leave. Moreover, the necessities of national defense functioned to erase or lessen most of the differences that had been anticipated by the Confederacy's founding fathers. As a corollary, Professor Amlund points up the expansion of positive government, particularly at the state level, in an effort to meet wartime demands upon human and natural resources.

The book touches upon most aspects of intergovernmental relations in the Confederacy. After delineating the constitutional issues and the principles applied in the South to meet them, there follows a discussion of the presidency focusing on the exercise of presidential power by Jefferson Davis with his sometimes conflicting combination of rigid constitutionalism and Southern nationalism. The Confederate Congress and judiciary are treated, particularly in regard to the continuity of institutions, procedures and personnel from the "old Union." Separate chapters are devoted to finances, administration, and confederate-state relations. Amlund's data are derived from historical studies of the Confederate government and its leaders plus published documents of the C.S.A. By combining both kinds of sources, the author is able to add some sense of actual occurrences to the bare bones of legislation and court decisions, though the book's brevity and the lack of examination of primary sources preclude the development of supporting case studies, which would have been most useful in underscoring the points he makes.

This reviewer can hardly take exception to the book's thesis and conclusions. All the evidence he has examined points in the same direction. However, the author overlooks some complicating factors which need to be considered in assessing intergovernmental relations in the Confederacy. The division of opinion between Southern nationalists and orthodox states-righters, which outdated secession, continued as an important aspect of Confederate politics and was found at the root of most of the confederate-state conflicts that did occur, is virtually ignored. The problems of congressional and presidential weakness generated by the lack of a party system are not explored. Perhaps most significant, the problems of dual versus cooperative federalism are totally ignored, either as part of the intergovernmental setting out of which the Confederacy emerged or as an aspect of intergovernmental relations in the Civil War South. From reading this book, particularly in light of William B. Hesseltine's admirable *Lincoln and the War Governors,* one gets the strong impression that, while the North was capitalizing upon a *de facto* tradition of federal-state cooperation and deepening cooperative patterns as part of the war effort, the South was attempting to implement a theory of dual federalism against all practical reason, a fact which contributed in its own way to the downfall of the Confederacy. Finally, the very exciting question of bureaucratic persistence through political crisis is hinted at but not substantially raised.

Professor Amlund's book is a welcome addition to the small shelf of political studies of the Civil War. It does not attempt to exceed its self-imposed limitations. Accordingly, it raises many questions for further and deeper research into the problems of a federal system in crisis.—DANIEL J. ELAZAR, *Temple University.*

The Faces of Power: Constancy and Change in United States Foreign Policy From Truman to Johnson. BY SEYOM BROWN (New York: Columbia University Press, 1968. Pp. 397. $8.95.)

The need for another book that reviews the major decisions of United States foreign policy in the post-World War II period might be questioned. But this book is an analysis with a difference—an important difference. Seyom Brown discusses United States policy from the perspective of how decision-makers in the United States viewed their adversaries and the alternatives as those decision-makers saw them. The author does not attempt to write a series of case studies, thus moving from one crisis to another, as is so often the case with foreign policy reviews.

The reader who is looking for more detailed and factual information about the many crises in which the United States has been involved since World War II will be disappointed. But if the reader is looking for an intelligent, well-reasoned interpretation of the patterns involved in foreign policy-making in the United States, this book is well worth the effort of a careful reading.

Brown recognizes that the task of interpreting how decision-makers perceive a situation is a difficult one. He admits that "any such analysis of the judgments of the nation's responsible policymakers is tentative and speculative. . . . Nor is there any logical formula by which one may infer, with a high degree of confidence, the basic assumptions underlying observed official behavior" (p. 3). But Brown feels that "the public record" provides enough information that inferences about behavior can be made. Brown carefully points out that his only source of information is "the public record." Since the author wrote this book while on leave from his position on the staff of the RAND Corporation, his reference to the public record as his source of information is undoubtedly necessary. However, it is a fair assumption that the classified information Brown had available to him as a staff member of the Rand Corporation does not contradict "the public record" that he employs.

Brown begins his review of U.S. foreign policy by attempting to identify the "irreducible national interests" and the major premises on which U.S. policy is developed. The author recognizes the conceptual problem in using a term such as "irreducible national interest," but defends his use of the term as being necessary since policymakers debate at such length what this term encompasses and act as though such a no-retreat position exists. In effect, Brown defines "the irreducible national interest" situationally as whatever decision-makers feel the term means in a particular crisis. In his discussion of the various crises of American foreign policy, Brown, however, makes very limited reference to what is regarded as the "irreducible national interest" in those particular situations.

While some reference by Brown to the "irreducible national interest" is understandable, certain other terms he uses with little or no explanation are more difficult to understand. He uses the term "balance of power" apparently to mean any distribution of power existing in the international system and particularly the one existing between the Cold War blocs. After the lecturing Professor Claude (*Power and International Relations*) gave political scientists over their slip-shod use of various terms including balance of power, more attention to the manner in which Brown uses "balance of power" is appropriate. The same criticism could also be leveled at Brown's use of "bloc politics." He uses the term without explaining the sort of international behavior a reader should associate with this sort of politics. Potentially, however, Brown's analysis of United States policy makes for an interesting comparison with the "rules" of bipolarity that several political scientists have attempted to identify. In light of the objectives of this book, however, a rigorous conceptual framework is not necessary and the imprecise use of certain terms is not unduly distracting.

One of the most interesting aspects of this book is Brown's development of the assumptions, particularly about the Soviet Union, held by major policymakers. His discussion of Averell Harriman, John Foster Dulles, Dean Acheson, and President Eisenhower in this regard is particularly well developed, as is his account of the relationship between Eisenhower and Dulles.

What seems to be a major shortcoming in Brown's book is the lack of attention to domestic as opposed to international considerations in the development of policy. It may indeed be the case that "the public record" in regard to foreign policy decisions is generally stated in terms of international considerations, but the influence of domestic considerations cannot be ignored even if the public record does not place the proper emphasis on domestic influences. The almost total absence of any mention of the influence of McCarthyism in the early 1950's or the domestic pressures that the United States not allow the Dominican Republic to become another Cuba are examples. Brown might have said more about the policymakers' assumptions about the domestic system than he did.

One of the "constancies" that Brown observes in United States policy is that "each Administration [relied] heavily on the tools of physical coercion —sometimes as a visible and active ingredient in a particular situation—in order to control external threats to our national interests" (p. 365). This, Brown concludes, is constant during the past four presidential administrations. Since his analysis throughout the book has been to deal with crises such as Berlin, Laos, Vietnam, Cuban missiles, Dominican Republic and such, what might be a more appropriate conclusion is that the United States is most apt to express its physical power during a crisis than at other times and the more serious the crisis the greater the reliance on physical power. Brown has not, as he suggests, provided evidence that the United States relies so extensively on "physical coercion" in more routine diplomatic exchange.

It would not be difficult to challenge Brown's interpretation of the public record in drawing conclusions about the assumptions and alternatives in regard to various policy choices, but his analysis, even when debatable, is always well-thoughtout and carefully presented.—DAVID N. FARNSWORTH, *Wichita State University*.

Government and Slum Housing: A Century of Frustration. BY LAWRENCE M. FRIEDMAN. (Chicago, Illinois: Rand McNally and Company, 1968. Pp. 216. $8.00 cloth; $2.95 paper.)

Lawrence M. Friedman, a professor of law, has written a scholarly book dealing with the "authoritative promulgation of housing legislation" by Congress. His concern is with only a part of the housing policy process, and he points to the "dark stain of failure" which covers the way in which legal institutions have been brought to bear on this social problem.

He begins with the "negative" public programs —the path taken by tenement house laws, housing codes, etc. In the discussion of "positive" programs he emphasizes public housing. Next Friedman considers urban redevelopment and renewal, "uniting recent movements of mighty proposition in many elements of the past." He assesses proposals and programs which "constitute the opening wedge of the future," before he concludes with the lessons of housing history.

Defining slum housing in terms of its physical standards and in terms of the social conditions of slum neighborhoods, Friedman incorporates the two elements—physical and social—in his theoretical approach to an analysis of the problem. The social-cost approach and the welfare approach are challenged by people who believe that the slums are beneficial not only to society but to the people who live in slums. Their argument ends in strict residential segregation or apartheid. It is obvious, says Friedman, that race and income segregation have played powerful roles in preventing the passage of adequate kinds of housing legislation.

Public housing, the only alternative, is the government's answer to slum housing according to the author. The government can build decent, respectable houses and apartments and rent or give these to the poor. Since public housing has not worked magic, Friedman claims, it has been deserted by those who care less for the welfare of the inhabitants than for the assumed social costs of bad urban planning. If public housing needs to be reformed—and he assures us that it does—his suggestion is that one might ask tenants and potential tenants what should be done.

Working within a "limited" group theory of politics, Friedman says, that diverse "interests" are responsible for the making of housing law. They are direct pocket-book interests (downtown merchants), institutional, material, emotional, status, as well as reform interests. Direct materials interests are more powerful than reform or ideological, which are insufficient to pass major housing legislation. Since people's perception of their interests are important factors in determining their behavior, the reformers must define or redefine the situation—the need for public housing—in such a manner that major economic and social interest will not oppose it.

Friedman also assures us that governmental institutions affect political behavior and play a large role in maintaining housing laws. Through federalism administration can be local and give free play to the operation of experts, professionals, and civil servants, somewhat independently of municipal politicians. The complex, mixed federal-state-local nature of housing law presents reformers and pressure-groups with problems and opportunities. Urban renewal is not easily diverted or destroyed. It has many ways to disarm the opposition; promises, beautification, humanitarianism, abolish poverty, and money.

Despite the setbacks of the past, Friedman assures us there are several reasons for guarded optimism. The militancy of the civil rights movement has spilled over into slum housing area, and through militant action the poor Negro may persuade the white, middle-class majority that its own self-interest requires concessions. The reform forces, as well as the academic community, the churches, H.U.D., and the model cities program are engaged in mobilizing resources toward a solution of the problem.

Friedman believes that genuine reform through legislation is politically possible; the housing problems of the poor can be solved. The government, however, must use carrots not sticks. The country must redefine its goals: it must decide to house the poor. The level of public housing construction programs has never been high enough to fill the need.

He claims that the boldest way is also the simplest; vastly to increase the subsidy and radically to diminish the punitive element in housing legislation. We ought to "swallow our distaste and look for ways to make it profitable to own houses in the slums, to make it as profitable as selling bread to the poor or showing them movies, which are activities carried on by respectable people." Rent subsidy is a timid step toward encouraging construction of housing for the poor. It could be improved, emboldened, and expanded.

The dominant motifs of Friedman's book on slum housing become: concern for the quality of life, the need for getting the people into the act, and confidence in the efficacy of the private sector in social endeavors. The Johnson housing bill of August 1, 1968, includes the same ideas; interest-rate subsidy for the home ownership program, a subsidy-rental program, a new Federal-private insurance program, an accelerated public housing program, more urban renewal, a "new towns" program, and a "housing partnership" program which will attract wealthy individual and corporate investors.

Because of the focus of his inquiry, Professor Friedman necessarily had to give a limited analysis of the politics of public housing policy process. I would suggest that Friedman's historical survey leaves less room for optimism than he indicates. Policy and process are inseparable; to change the policy, the process must be altered. This is true when change is incremental, much less fundamental. A group theory of politics almost by definition leaves little confidence that our housing goals will be clearly "redefined" as Professor Friedman asks and even less confidence that the public housing policy process will produce the "simple" incremental solutions which he proposes. If the public housing policy process is a "group process" as he suggests and it has produced legislation which has "failed" to solve the problem of slum housing to date, I see no reason why we should have confidence that the traditional ways of doing things will redefine the situation. The "private-sector" approach has proven to be a deadend for solving the problem. More of the same seems inadequate.—MYRON Q. HALE, *Purdue University*.

Aid, Influence, and Foreign Policy. BY JOAN M. NELSON. (New York: The Macmillan Company, 1968. Pp. 149. $1.75, paper.)

The Strategy of Foreign Aid. BY LLOYD D. BLACK. (Princeton: D. Van Nostrand Company, Inc., 1968. Pp. 176. $5.95.)

The titles of these two books do not indicate the significant difference in subject matter, approach and purpose of the two authors. Joan Nelson's *Aid, Influence, and Foreign Policy* is a perceptive, conceptual analysis by an "insider" of aid policy-making in the Agency for International Development. After a brief historical introduction, the first half of her book considers the Agency's goals, programs, and program planning processes; the second half is an analysis of the influence of A.I.D. programs in improving recipients' development policies, achieving short-run U.S. political purposes, and promoting long-run political development in recipient countries. Lloyd Black's *The Strategy of Foreign Aid* discusses aid on a much broader scale, outlining United States, Free World, Communist bloc, and multilateral aid programs as well as the problems and needs of recipient countries. With this scope, the book becomes a useful overall compendium of facts and figures on aid programs, and a summary review of concepts, processes, and problems of economic development.

Miss Nelson views herself as a "rapporteur of ideas developed from 1962 to 1966 by past and present members of the Program Coordination Staff of the Agency for International Development." Dr. Black hopes to provide "a comprehensive factual tableau of the economic aid situation which will make the wheels of intellectual curiosity turn a bit faster." The first book is essentially conceptual, would serve as an excellent supplemental text, contains a single chart. The second book, though primarily factual, raises many of the problems and policy issues of development. It would be particularly useful for reference, contains 15 tables, 8 maps, and two graphs, and is footnoted and indexed.

In introduction, *Aid, Influence and Foreign Policy* highlights the changing geographic allocation of U.S. aid funds over time from Europe to the Far East, to the Near East and South Asia, and finally to growth of assistance to Latin America, citing U.S. aid levels of $3.41 billion for 1949–52; $1.82 billion for 1953–57; $1.85 billion for 1958–61; and $2.3 billion for 1962–66. Thus, the 1968 cuts in aid tend to restore the 1953–61 level. Roughly two thirds of A.I.D.'s effort in recent years has gone for economic, political, and social development, about one fifth for immediate defense or security problems, and a lesser amount for "humanitarian" purposes. Miss Nelson notes a tendency within the Executive Branch to downgrade the importance of the inevitable conflicts between these A.I.D. objectives and the negative side-effects of their interaction on the success of programs.

Then the author discusses the criteria for allocation of A.I.D funds, the degree of dispersion of funds, program planning principles, and major limits on the effectiveness of A.I.D programs. The three basic criteria considered in allocating funds are said to be the political importance of the country's development to the United States, the ability of the country to use aid effectively, and the availability of resources from other sources. Aid went to 74 countries in FY 1966, but the author argues that it was not so dispersed that it lost effectiveness, pointing out that half of all funds went to 9 countries, that 35 to 40 others received a total of only two per cent of A.I.D. funds for limited objective programs. Concerning program planning principles, Miss Nelson notes that three are accepted and practiced: Programming by country or region, concentration on a few high-priority goals, and identification of self-help measures to be taken by the recipient country. Accepted as principles but practiced less effectively, she says, are long-run planning and systematic analysis of alternatives. She sees the major limits upon achievement of A.I.D.'s program objectives as being the Agency's multiple and conflicting objectives, the necessity of working with independent governments, and the relatively small size of the U.S. fund imput compared to the recipient nations' own development efforts.

The second half of the book discusses the use of economic aid in each of its three forms (technical, capital, and commodity assistance) for improved development policies; short-run political purposes, and long-term political development. This discussion of the use of aid to gain influence or leverage is a valuable contribution to the knowledge of "outsiders," because it gives specific illustrations of the impact of aid upon recipients' domestic and foreign policies. In conclusion, the author calls lack of attention to political development problems the "missing dimension in U.S. foreign policy." She recommends that A.I.D strengthen its capacity to assess these problems. Her approach throughout this carefully written and well-organized book is moderate, flexible, and objective.

The two most useful chapters in *The Strategy of Foreign Aid* are those on "The Recipients—Problems and Needs" and "The Donors—Dimensions, Scope, and Volume of Aid." Together they comprise 50 per cent of the book. Discussing recipients, Dr. Black raises "the obstacles, bottlenecks, and roadblocks" to development. As a geographer, he naturally starts with the physical environment, then presents problems of health, population, food, illiteracy, infrastructure, one-crop economics, trade gap, poverty, debt-servicing, management, and absorptive capacity. Emphasizing U.S. programs in treating donors, Dr. Black summarizes the programs of twenty-four other "Free World" bilateral aid programs before discussing "Sino-Soviet Bloc" and multilateral aid. Somehow, the use of the old bipolar dichotomy seems out of data in a more polycentric period. Discussing the "real reasons" for foreign aid in an earlier chapter, the author appears to accept as valid the idea that the leaders of the Soviet Union and Communist China are "uninhibited by moral or humanitarian considerations." This may be a misreading of what was intended, because similar strictures upon perception of the environment in which foreign aid functions are not apparent elsewhere in the book. Aid policies need to adjust not only to changes within recipient countries but also to those in the broader climate of international relations.

Dr. Black includes in his book a useful listing of what he calls "weaknesses, especially relating to the utilization of personnel in A.I.D." Drawing upon the views of A.I.D. personnel, he says that A.I.D decisions involve the representatives of too many agencies; field personnel are burdened by having to respond to too many requests for reports; mission directors do not have enough independence of action; field personnel must spend too much time with VIP visitors; personnel receive insufficient orientation before going overseas; officers on Washington assignment are rotated through too many jobs in a single tour; the Agency lacks a process for retrieving information from its past studies; the Agency's economists don't recognize the value of insights from other disciplines; foreign personnel brought to the U.S. by A.I.D. training can't find jobs when they return home or want to remain in the United States. In fact, many of these or very similar problems also concern the other U.S. foreign affairs agencies and some of the U.S. domestic affairs agencies with overseas interests.

Although *The Strategy of Foreign Aid* tends to be a rather comprehensive dictionary of foreign aid agencies and programs, it also raises many issues concerning aid policy which the student with "intellectual curiosity" will wish to pursue further after reading Dr. Black's helpful collation of facts and ideas concerning the "economic aid situation."
—Robert E. Elder, *Colgate University.*

Class, Race, and Labor: Working-Class Consciousness in Detroit. By John C. Leggett. (New York: Oxford University Press, 1968. Pp. 252. $7.50.)

This study of working-class consciousness is based on a sample of three hundred seventy-five workers interviewed in 1960 in seven Detroit neighborhoods selected to provide similar numbers of Negro, East European, and Northwest European workers. The operational definition of class consciousness is a Guttman scale which classified without error 75 per cent of the respondents. Instead of relying on the individual's self-perception of his class identity, the usual indicator of class consciousness, Leggett's index consists of spontaneous use of class terms, a belief that profits go to businessmen when the economy prospers, stated willingness to engage in direct action, and advocacy of economic equality. This index apparently differs from simple class identification, for Leggett says at one point that "a high correlation" between his index and class self-identification "does not occur" (p. 216). Yet the author neither justifies his measure of class consciousness nor does he compare the differing correlates of the alternative indices.

This is significant only because Leggett implies a critique of the "many social scientists" who, "during the 1950's . . . abandoned their Marxian assumptions and turned to an examination of why U.S. workers lacked militant class attitudes" (p. 27). Leggett does little to make his critique more explicit. Did they use wrong measures of class consciousness? Did they code responses to open-ended questions incorrectly? Leggett seems willing to code as evidence of class verbalization responses that Campbell *et al.* in *The American Voter* (1960) would have coded relatively low in their index of capacity to conceptualize in ideological categories. Or did they study the wrong cities? Leggett seems to believe that Detroit, where social and political life is particularly conducive to the development of class consciousness, is the ideal location for such an investigation. Perhaps Leggett remains vague on these points simply because his data cannot demonstrate how important class consciousness is for working class, social and political life in general. He is limited to comparisons among subgroups in his working class population; any general interpretation inevitably rest upon the observer's judgment and his own definition of a *significant* level of class consciousness.

Even though Leggett is not able to resolve this question, his concern with the topic did spur him to explore the sources of whatever consciousness does exist in Detroit. Of particular interest, he finds greater consciousness among recent migrants to the city than among those born in an industrial center, even when ethnicity is held constant. This finding not only contradicts one of Marx's major contentions but also, as Leggett notes, helps to explain the greater consciousness in Nineteenth than Twentieth Century European societies. In addition Leggett finds that unemployment correlates negatively with consciousness among Negroes except when the black worker is unionized. He also demonstrates that consciousness and union membership is related to voting for such liberal Democrats as Governor G. Mennen Williams. He makes the reasonable argument that such a relationship is contingent upon a liberal candidate backed by labor support running against a conservative. Had Leggett asked questions about other elections—particularly, about Detroit's local, non-partisan elections—more evidence for this assertion might have been possible.

Finally, he notes that consciousness is much greater among Negroes than among whites. Here it is worth noting the decisive effect that the recent racial crisis has had on social scientists. Even though Leggett began this study as a critic of the consensus school, his own interpretation of American society changed in the course of writing the book. This is revealed by the differences in the tone of the body of the book, which is largely based on previously published papers, and the recently written epilogue. In the analysis itself, the Negro community is treated as another ethnic group a step behind other groups in its integration into American society. In the epilogue however, the problems of blacks are so severe, so unique, that it is possible to imagine that Negroes, unlike other ethnic groups, may come "to prefer *insurrection* to voting" (p. 152). Similarly, whereas in the body of the book industrial unions are complimented for their contributions to class consciousness, a major subsection of the epilogue is entitled "Is Labor Failing the Worker?" (p. 148). It is no longer the business community but "the C.I.O., . . . the Democratic party, and like established organizations" which are held responsible for the conditions of Negroes (p. 153). This new mood is symbolized by the abrupt transition in terminology from "Negroes" to the "black community" as one begins reading the epilogue. Leggett probably differs but little from many social scientists in his changing view of the country's social and political institutions; it was his misfortune to have reported research which spanned the rapidly changing seventh decade.

Nonetheless, focus on these limitations of the book should not obscure the fact that it remains a solid empirical contribution to our understanding of the sources and consequences of class consciousness.—Paul E. Peterson, *University of Chicago.*

The Public Library and its Community: A Study of the Impact of Library Services in Five Pennsylvania Cities. By William R. Monat, with the assistance of Lawrence K. Pettit and Philip M. Clark. (State College, Pa.: Himes Printing Co., 1967. Pp. 162.)

This little volume is the seventh published as part of the Pennsylvania state library monograph series, but is the first in this line to focus explicitly on the relationship between certain public libraries and their politico-socio-economic environment. This study's major objectives were ambitious in nature and five in number—(1) to determine who uses these libraries, with what frequency and for what purposes, (2) to determine the attitude of library users and non-users alike toward that institution which many people who have never set a foot inside regard as "a good thing to have around," (3) to ascertain the amount and sources of financial support for library services, along with the attitudes and knowledge of concerned citizens about these financial arrangements, (4) to determine, in some rough way at least, how well these libraries meet the "needs" of both their users and the communities they purport to serve, and (5) the determine where these libraries fit into the overall governmental scheme of things in their respective areas. In order to carry out this task in a very thorough and conscientious manner, the authors of this study concentrated their efforts on five medium-sized cities in Pennsylvania.

Unfortunately, for a variety of reasons, the results of their analysis can only be described as rather mixed. The findings as to the public's use of library facilities, for example, are probably the most reliable in this particular survey, though they differ relatively little really from those arrived at by Bernard Berelson (*The Library's Public*) back in the late 1940's—namely, that only about 20 to 25 percent of the adult population use the library much at all and most of these tend to be young, comparatively well-educated, female, and middle-class. Similarly, the vast majority of the respondents in this survey expressed general satisfaction with their library facilities and services. Just what interpretation should be placed on this finding, however, is a matter of doubt or opinion, for as the authors themselves observe, "the consumer of library services is not well-equipped to evaluate those services if he lacks standards or measures for rendering judgment. Not knowing what might or could be, the client is likely to conclude that whatever is, is good."

Perhaps the most significant set of data generated by this study has to do with the library users' (cardholders') knowledge of the level and sources of financial support for these five public library systems in Pennsylvania, together with an analysis of user attitudes toward increased taxation and the question of whether libraries should be financed through general funds or a special library tax. These are important topics rarely treated at any length in the professional literature. As one might almost predict, the authors found that there was a great deal of ignorance, even among concerned citizens, as to existing financial arrangements for library services. That the majority of those queried stated that they did not know whether the libraries in their communities were adequately financed or not, the authors construed as indicating the possible existence of a sizable bloc of latent support for library improvement or expansion programs, an assumption some might care to challenge. Nevertheless, one point was made very clear by the respondents. They did not choose to pay for better library services through any kind of special taxes; all such funds must come out of the general treasury or be raised by other levels of government.

The question of how well the "needs" of both the users and non-users have been met by these libraries is, of course, a tricky one, and one not dealt with very satisfactorily in this volume. Nowhere is this problem treated at any length. Perhaps the authors should not be judged too harshly in this regard, considering the paucity of productive effort of other researchers in the field. As it stands, the closest the authors come to providing any sort of answer here is by equating need with public and consumer satisfaction.

The authors' attempt to deal with public libraries as governmental units left something to be desired too. On the basis of their data, they concluded that because of the nature of library financing and their pattern of historical development, public libraries are not regarded or treated as agencies of local government, a point which is probably true. They also claim that the two most important factors in determining the actions of library boards were the confidence the board had in the professional competence of the chief librarian or library administrator and, secondly, in the extent to which the library depended on local public funds to finance its operations and the nature of that relationship. However, inasmuch as this study is based on an analysis of five public library systems, it is difficult to tell just how much stock should be put in such generalizations; perhaps very little. Moreover, this Pennsylvania survey contains virtually no information about the web of library group relationships in contrast, for instance, to Oliver Garceau's *The Public Library in the Political Process,* a work which is in many ways still clearly superior. Thus from a political science standpoint in particular, this book is of only limited value.—PHILIP H. BURCH, JR., *Rutgers, The State University*

Illinois Municipal Finance: A Political and Economic Analysis. BY GLENN W. FISHER AND ROBERT P. FAIRBANKS. (Urbana: University of Illinois Press, 1968. Pp. 242. $7.50.)

To date there have been very few studies focusing on municipal finance within only one state. The value of this approach is that it eliminates the disturbing effects of variations in functional responsibilities of municipal fiscal systems found in other states. Therefore, *Illinois Municipal Finance* is a welcome addition to the literature.

The main effort of the book concentrates on a detailed discussion of Illinois municipal property taxes, tax limits, non property revenue, and indebtedness using 1962 data in the main. Two major criticisms of the Illinois municipal finance system presented in the study concern assessment practices and tax limits.

In the case of assessment the authors point out that even though the Illinois State Department of Revenue was instructed in 1945 to assess property at full value, "Revenue Department Figures show that the ratio has declined from about 75 per cent in 1945 to 48 per cent in 1957" (p. 77).

In terms of tax limits the authors express concern over the usefulness of this legal constraint. "Evidence of what the rate limits happen to be at a given time is not easy to find, and once found, it is difficult to evaluate" (p. 93).

In addition, the authors reveal that the confusion over tax limits works for the benefit of the municipalities which regularly extend these limits causing railroads and other industries to pay their taxes under protest as a matter of police. "Industries other than railroads often pay their taxes under protest too" (p. 94).

The major weakness of this study lies in its methodology. First of all the study would have been considerably enhanced if it took a more longitudinal approach in an effort to trace the development of the system. In the few attempts that were made to use longitudinal data, such as Table 36 illustrating gross debt outstanding from 1902–62, no effort was made to put the dollar amounts in constant dollars which would have given greater meaning to the purpose of the tabulation to say the least.

The third chapter, "Municipal Expenditures," is by far the weakest section of the study. Although a great deal of information is presented in this chapter, it is very descriptive with little effort being made in the direction of meaningful analysis.

Many statements are made which have little documentation. For example the observation that "analysis of data for individual cities reveal that there are substantial differences in expenditures per capita among Illinois cities" (p. 59) is made despite the fact that neither the standard deviation nor the coefficient of variation is ever presented to substantiate the claim.

The discussions of the various individual expenditures are really rather trifling, *e.g.*, three quarters of a page is devoted to the discussion on parks and recreation expenditures not including a one page table accompanying the discussion which rated only one paragraph of comment. The authors conclude the third chapter with the observation that economic and political characteristics of the cities may be related to expenditure decisions (p. 63).

Why did the authors completely overlook the regression equations in Appendix A when discussing expenditures? On the whole the regression analyses showed that per capita assessed valuation was the most important determinant of the expenditures analyzed with various demographic measures and tax limits being secondary but significant predictors. It seems as if the authors are more concerned about straddling both the traditional and more empirical methodologies in the interests of harmony than they are in solid research.

The unusual presentation of the regression analysis in the appendix is defended on the ground that the authors wanted ". . . to avoid the impression of overemphasis on the importance of multiple correlation analysis" (p. 211). In addition, the discussion is prefaced by seven criticisms of multiple correlation analysis.

It is, of course, true that regression shows only correlation and not causation. Based on correlation, however, the analysis does show relative importance of variables as well as the direction of association including level of confidence that this is not a random association. In addition the percent of the variance explained by the equation is given plus a wide range of other relevant information which space precludes from discussing.

The problem of intercorrelating variables can be handled in the stepwise regression. Basically the researcher must establish what level of intercorrelation is unacceptable and eliminate all variables interacting above this level which are least important to the equation stepwise. While this method does not solve all the effects of interaction, it does make the analysis more manageable and meaningful.

The seven objections made to regression analysis do raise pertinent issues. But too much can also be made of the shortcomings of the technique. There has been a host of literature in recent years employing multiple correlation analysis to good advantage in reaching greater understanding of the dynamics of the municipal fiscal system. Research methods cannot be rejected because they are complex and do not answer all questions with absolute certainty.

While this study will not have great interest to the political scientist being more economics in tone, there will be interest for the specialist in the Illinois municipal fiscal system. However, a more systematic methodological approach could have resulted in a significantly more valuable study in terms of theoretical interest and positive findings. —STEPHEN W. HARTMAN, *University of Connecticut.*

Toward Equal Opportunity: A Study of State and Local Antidiscrimination Laws. BY DUANE LOCKARD. (New York: The Macmillan Company, 1968. Pp. 150. $1.75 paper.)

The racial crisis lies at the heart of America's urban problems today. Negro-white antagonism and conflict over housing, public schools, and community order prevent the development of comprehensive solutions to the problems of our cities. State and local governments are unable to alleviate these conflicts either because of long-standing institutional barriers to change or simply because of a defense of the white community to preserve the status quo at the expense of Negro demands.

We are living through a time of confrontation politics in our cities: whites and blacks are polarized over crucial matters of public policy. This is especially true in transitional lower middle class white neighborhoods bordering expanding Black ghetto areas. During a time of confrontation, it is difficult to maintain communications between the races. While Black leaders are employing a variety of direct action techniques to dramatize the need for change, proponents of "white backlash" argue that Negroes are trying to change things too fast by means of integration. What Black leaders really want is community control over ghetto institutions. As the communications gap widens, there is a real possibility that the confrontation politics of today may yield to the politics of repression of tomorrow. Black ghettos may become permanent armed garrisons patrolled by the National Guard. (Wilmington, Delaware is already experiencing this.)

Duane Lockard's book illustrates the inadequacies of the traditional, civil rights-oriented approach to Negro inequality in America. State and local antidiscrimination laws in employment, housing, and public accommodations did not benefit the mass of impoverished ghetto Negroes. The legal approach, however, was the only public response to racial discrimination that could be expected from state and local political systems. Such laws were an attempt to provide equal opportunity for individual complainants through a slow process of adjustments by means of administrative machinery.

What, then, are the values of studying state and local antidiscrimination laws? Lockard does show that it is necessary to understand the complexities of the legislative process before analyzing the impact and effectiveness of such measures. In Chapter 2, he clearly identifies the actors in the legislative struggle, together with the strategies and tactics employed to promote or block action.

Secondly, the author is realistic in evaluating the administration and impact of employment, fair housing, and public accommodations laws in Chapters 3, 4, and 5. He notes that antidiscrimination laws rely mainly on individual complaint initiative rather than agency prosecution of discrimination; that agency procedures are complex, time-consuming, and involve much delay; that agencies often lack injunctive powers to retain jurisdiction over disputed jobs or property during proceedings; and that agencies are often timid in the investigation and prosecution of cases. Even more important, antidiscrimination measures are used primarily by middle class Negroes who oftentimes become discouraged since most employment and housing disputes are not settled satisfactorily.

Lockard recommends that the full potential of these laws should be tapped by improving administrative procedures. Agencies should attempt pattern-centered enforcement in entire industries and neighborhoods as well as informing ghetto residents of the availability of services. Furthermore, legislatures should provide adequate funding and staff services so that the agencies can carry out their legal mandates. While pessimistic about the ability of antidiscrimination laws to assist the entire Black community, Lockard claims that such measures can at least aid in reducing overall patterns of discrimination.

The book falls short, however, in not discussing the "new equality" presently sought by Black leaders in the ghetto. It seems that community control over ghetto institutions is the key to understanding Negro drives for equal opportunity today. Black inner-city residents must have the resources to attain meaningful political power before they can compete on equal terms with the majority white community. As a young Negro medical student recently told me, "Civil rights activity moved the Black community from hopelessness to hope; Black power is the means by which the Black poor will move from a position of powerlessness to power." If this is true, then it may be possible to test the effectiveness of democracy at the grass roots as a viable goal for the Black community. Such political power will perhaps enable new ghetto representatives to define the terms under which future legislative struggles will be fought to eradicate all discrimination in jobs, housing, and education.—ALAN SHANK, *Boston University.*

Crisis in Print: Desegregation and the Press in Tennessee. BY HUGH DAVIS GRAHAM. (Nashville: Vanderbilt University Press, 1967. Pp. 338. $7.50.)

This is a straightforward and thoroughly readable description of the response of the press of Tennessee to matters racial from the momentous *Brown* decision to the mid-1960's. Graham's method is to take the major race-related developments originating in or intruding into Tennessee politics—including the school desegregation decisions, the Civil Rights Acts of 1957, 1960 and 1964, and election contests between candidates of different racial persuasions—and to describe the editorial reactions of the various Tennessee newspapers to these. Although he surveyed the 122 weekly and semi-weekly, and the 27 daily papers in the state, Graham devotes most of his attention to the eight big dailies, the *Journal* and the *News-Sentinel* of Knoxville, the *Times* and *News-Free Press* of Chattanooga, Nashville's *Banner* and *Tennessean,* and the *Commercial Appeal* and *Press-Scimitar* in Memphis.

Crisis in Print is not likely to startle even the casual observer of southern politics. What do we learn? (1) Just as Tennessee racially is one light year away from Alabama and Mississippi which it borders, so the response of its papers to the assault on the old biracial system in the decade after *Brown* was decidedly different from that of the press in its deep-South neighbors. Tennessee is a rim-South state. Its eastern third has few Negroes; the entire state was only 16.5% Negro in 1960 (in contrast to 42% in Mississippi); two of the seven southerners in the U.S. House of Representatives to vote for the Civil Rights Act of 1964 were Tennesseans (with Texas and Florida supplying the other five supporting votes). It is hardly surprising that the state's newspapers didn't display single-minded opposition to desegregation, and that some indeed gently applauded the big civil rights gains of the period. Four of the eight dailies listed above, for example, editorially supported the 1964 Civil Rights Act. (2) If Tennessee isn't Mississippi in its racial tradition, neither is it New York. The generally pro-civil rights papers of the state frequently saw fit to restrain their enthusiasm. The Chattanooga *Times,* the senior member of the Ochs family of papers, was far more guarded in its endorsement of the *Brown* decision than its more esteemed New York relative, quite likely out of some assessment of the tastes of its Chattanooga readers. (3) Graham reveals that the position of a Tennessee newspaper on civil rights controversies in the post-*Brown* decade could pretty much be predicted from its general position on a "progressive-conservative" continuum. That is, the four otherwise progressive dailies among Tennessee's "big eight" gave their editorial blessings to the end of Jim Crow, and the conservative papers regularly voiced displeasure.

One of the reasons why *Crisis in Print* isn't of greater value to the political scientist is that it was written for a much more general readership. In the Introduction, the author voices the understandable dismay of a liberal-minded southerner that many northerners insist on seeing the South as an undifferentiated stomping ground of Tillmans and Bilboes in the past and now of Thurmonds and Wallaces. He thinks that the case of Tennessee and specifically of the Tennessee press demonstrates clearly that this just isn't so. Well, of course it does, but hopefully not too many students of southern politics needed to be freed from such a delusion.

I wish Professor Graham had paid at least some attention to two important questions. First, newspapers live on advertising. How much, and in what ways, were editors influenced in their commentary on civil rights developments by assessments of what advertisers would do? We know about the economic sanctions imposed through the withdrawal of advertising in deep-South states. The process would almost certainly have been more subtle in Tennessee, but did this occur at all? Had Professor Graham explored this, he would have given us a valuable note on the role of businessmen in southern desegregation.

I miss as well in *Crisis in Print* any attempt to assess the impact of the press on the public response to racial change in Tennessee. Graham assumes that newspapers reflect the pressures of the settings in which they are published, and at the same time that they have some influence on these settings. So do I. But the matter cannot be left here, even though it is not an easy one to handle. Graham does leave it as a casually stated assumption, and he in turn is left with routine reporting of what the newspapers said editorially. I am afraid most political scientists will feel that does not make very good copy.—EVERETT CARLL LADD, JR., *University of Connecticut.*

COMPARATIVE GOVERNMENT AND POLITICS

Economic and Political Development: A Theoretical Approach and a Brazilian Case Study. BY HELIO JAGUARIBE. (Cambridge: Harvard University Press, 1968. Pp. 202. $5.75.)

Though the two studies in this volume are presented as revised and updated English versions of essays written in 1960 and 1961, they are still best understood in terms of the intellectual mood of the beginning of the decade rather than that of the late 1960's. That mood, especially in Brazil, was one of buoyancy and mild euphoria in view of the apparent success economically and politically of an affirmative nationalist development policy that flew in the face of orthodox monetary doctrine and defied the established international and United States monitors of national growth plans. The opening essay on "National Development and Political models," though it strives for a sober and hard-headed approach to the definition of available development models and the options among them, was nevertheless written at a time when the range of models, if not unlimited, was at least seen as reasonably open and perhaps expanding. Clear thinking, firmness, and political acumen among a few visionary politicians, entrepreneurs, and social technicians might still prevail against the irrationalities of international power tensions as well as reaction, timidity, and political primitiveness at home. Interim events have justifiably dimmed confidence in the will or capacity of present elites to achieve more satisfying national futures through planning and the elaboration of highly rational ideologies.

It is therefore important in judging Jaguaribe's work to maintain some perspective on those things which were and remain lasting contributions to thinking about development in Latin America (the stress on purpose, foresight, and choice; the insistence on viewing development as a total process; the rejection of growth as against qualitative change as a standard for development gains; the redefinition of development in terms of the realization of maximum potentialities at a given historical moment rather than as a fixed ranking on some array of productive capabilities). Against these noteworthy contributions, which may seem less fresh today than a decade ago, a number of negative features bear mention. These include the pervasiveness of an elitist rationale (an over-reliance on social science expertise, charismatic politicians, and enlightened entrepreneurs), the emphasis on elite-mass cleavages and the spotty attention given to intra-elite dissensus, the cursory treatment of mass participation despite the central role such participation is given as a focal transition in political development, the absence of any concrete clues to action for planners despite the determined policy-oriented stance espoused by the writer. In addition political operations in the developed world that is implicitly taken as the ultimate goal are at more than one point ingenuously portrayed if not outrightly idealized. The alleged workings of elite consensus on goals as well as the presumed effects of participation on mass loyalties and motivation suffer particularly from an uncritical depiction in which the very aspects that are most problematical are taken as given. On the other hand Jaguaribe's argument with respect to the viability of nations clearly prefigures one of Servan Schreiber's main theme in his recent *Le Defi Americain,* though the Brazilian volume emphasizes the quality of political leadership rather than managerial talent. Still the writer anticipates Servan Schreiber in calling attention to the need for social as against technological invention and innovation, particularly as regards the legitimation of strong political leadership.

Perhaps because Jaguaribe looks to the qualities of leaders and nationalist ideologies as the principal foundations of political legitimation, his review of the Brazilian case tends to find strong direction and national awareness where other analysts have found mainly drift and improvisation. Since the balance of new data as against novel interpretation is heavily tipped in favor of the latter in the compressed historical overview in the second essay, it is hard to say how strong a case can in fact be made for the existence of leadership elements with a well-formed national conscience reaching back into the colonial period. In any case this effort gives a curious slant to the historical portions of the country study. On the one hand Brazilians in the mass figure hardly at all in the analysis (slavery is mentioned for the first time when its abolition is noted). Secondly, qualities of foresight and nationalist purpose are attributed to early political leaders (especially, Dom João) that even noted apologists for these individuals have failed to claim for them. Finally, a mysterious nationalist conscience is alleged to have operated at critical moments among a select, enlightened elite. This conscience maintained "an unspoken constitutional pact" in the time of Dom João, allowed Dom Pedro to function as the "caretaker" of the unrealized possibilities of the nation during the 19th century, and is invoked as the critical underpinning for the new entente captained by the national bourgeoisie that is still depicted here as Brazil's main hope.

Though the author argues forcefully for an abandonment of irrational commitments of any order, there is a certain compulsiveness in his final insistence on a "Neo-Bismarckian" route to development despite his careful documentation of the abortive record of this line in Brazil itself. The political dilemma of development is clearly stated: the politician who is too moderate loses mass support; he who courts the mass will be toppled by panic and resentment on the right. Many, who unlike Jaguaribe now seriously doubt that national bourgeoisies can be awakened in time to save their own skins or appreciably change the life situation of the mass of Latin Americans, will want to take a longer look at the more revolutionary options that the author tends to dismiss as unrealizable.—FRANK BONILLA, *The Ford Foundation.*

Scandinavian Political Studies, Vol. II. EDITED BY PERTTI PESONEN. (Helsinki: The Academic Bookstore, and New York: Columbia University Press, 1967. Pp. 333. $8.00.)

This volume has broad scientific relevance; much of its emphasis is upon making theoretical and methodological contributions to the study of political parties, and this review will focus upon these concerns. There are two articles on each of four sub-topics: party formation and politicization; ecology; behavior; allegiance and images.

The first contributions are to our knowledge of the early structure and developmental sequence of national party organizations and to the causes of communal politicization and its effects on local political culture. Ulf Torgersen compares in detail the establishment of Norway's Liberal and Conservative organizations in 1882–85. He notes a poor fit with Duverger's typology. Additionally, his finding that the Conservatives were the first to organize locally both confirms the hypothesis that electoral defeat is the most important stimulus to improved organization and debunks the myth that the Liberals introduced the institution of party while the Conservatives disapproved of such organization. There is evidence that, the greater is the competitor's threat, the more likely is organizational response. The fact that both parties, in the course of four years, were able to draw two-thirds of their voters into formal organizations leads Torgersen to examine the conditions of rapid organizational growth. His analysis leaves one unsettling impression: were the new organizations really spontaneously formed, or should the conclusions about the process remain tentative until we gain knowledge of any role played by parliamentarians and other national leaders? In communal politics, politicization occurred two decades later at the initiative of the Labor Party and in the wake of economic and electoral change. Treating politicization next as the independent variable, Torstein Hjellum shows that it leads to changes in recruitment, increases in participation, and expansion of communal activity. He concludes by offering a scheme of causal chains.

The two ecological studies differ in purpose and method. Jan Stehouwer engages in longitudinal analysis of the post-war years to explain why national party support in Denmark has remained stable despite substantial change in economic and social structure. Examining communes which vary in urbanization, occupation structure, and population growth, he offers conclusions about party stability both within and across ecological units. While there is great stability in the relationship of urbanization to the Social Democratic vote, this party's support decreases with very advanced urbanization but increases with new urbanization; Liberal strength is maintained by compensating for rural losses with urban gains. Stehouwer details the methodological alternatives and problems in the Danish context, showing awareness of the full range of possible analytic pitfalls. Refined political cartography with communal accuracy is the achievement of Onni Rantala, who focuses on Finnish regionalism. Rantala examines each party's internal regions, which bear classification by strength of support. He pinpoints each party's core areas and notes such structural characteristics as location of party offices and newspapers, as well as geographic features, which are suggestive in explicating the patterns of influence into surrounding areas.

Of the two chapters on party behavior, one is descriptive and empirical, the other purely analytic. To determine the level of agreement among parties and tendencies toward bloc formation in the post-war Danish parliament, Mogens Pedersen employs methods akin to those of roll call bloc analysis. His data are from the three-way voting divisions (Y, N, abstain) on 2,600 bills. An "index of distance" permits comparison across time, and changes of government permit analysis of three constellations of opposition. Pedersen finds agreement extraordinarily great and that consensus is greater with minority than with majority governments. He does not identify the issue bases of disagreement, nor does he directly identify what seems to be the most plausible explanation for both his findings, namely that the Radicals usually exploit their pivotal position to negotiate broad consensus toward their own policy preferences rather than to move their bargaining into the more limited arena of majority coalition government.

The purely analytic contribution of Gunnar Sjöblom begins with criticism of Björn Molin's method of explaining party decision-making in multiparty situations (Vol. I). Molin employed three variables: policy values, perceived opinion effects, and coalitional opportunities. Finding weaknesses in the operationalization of variables and the logic of explanation, Sjöblom concludes that Molin's work is nevertheless valuable as a paradigm if not as the purported "form of explanation". Sjöblom attempts to resolve some of his objections and refines the scheme of analysis.

The methodologically difficult task of establishing the impact of party allegiance upon opinion formation is the concern of Bo Särlvik. With data from four Swedish national interview surveys, he presents four tests of impact. Some of his evidence is quite striking. He finds that, in varying degrees, respondents adjust their evaluation of governing party performance and their attitude toward various issues to conform to the positions of the parties with which they affiliate. Differences in awareness of party position, attitude toward welfare state politics, and socio-economic status are Särlvik's keys to explaining variations in the impact of party allegiance.

The methodology for studying changes in party and candidate images induced by radio and television during a campaign is the focus of Kaarle Nordenstreng and Osmo Wiio's article reporting a method test study conducted during the 1966 Finnish election debates. They employ the semantic differential developed by Osgood, Suci and Tannenbaum, with bi-polar scales consisting of pairs of opposite adjectives which permit construction of profiles. Factor analysis, in the form of a new symmetric transformation technique, provides quantitative indication of semantic similarities and differences between two image structures and indicates varying meaning of individual scales with regard to these image structures. The authors find little media effect on politician images and that radio listening results in more dimensions of image change than does television exposure. It seems that party images are easier to change than politician images because they are vaguer and that radio, being the more "diffuse" medium, leaves more interpretation to personal experience. Mainly the media accentuate existing images.

What we can expect from Scandinavian researchers in the near future is set forth in country-by-country descriptions of current projects. The Swedes present a detailed and exciting prospectus for many-faceted research in local government by several universities. Their study is likely to have great impact, particularly given its $180,000 annual financing and the paucity of local level literature in nearly all countries. The Danes announce the establishment of a data archive on municipalities and describe their new ecological research on voting behavior. For Finland, where such research is longer established, there is a review of both the most recent ecological investigations and the 1966 election study based on survey research. Finally, Henry Valen and Stein Rokkan outline the various approaches and foci in Norwegian election research. As in Vol. I, there is a comprehensive bibliography of writings in political science by Scandinavian authors. Additionally, for those wishing to keep abreast of recent political developments within the four countries, there are short articles of description and interpretation.

In large part through these volumes, the Scandinavians are placing themselves in the mainstream of modern political science with theoretical and empirical contributions. These volumes are communications of significance. They merit international attention.—SVEN GROENNINGS, *Indiana University.*

International Aid to Thailand: The New Colonialism? BY RONALD C. NAIRN. (New Haven: Yale University Press, 1966. Pp. 228. Price: $6.50)
Politics in Thailand. BY DAVID A. WILSON. (Ithaca: Cornell University Press, 1966. Pp. 307. $1.95)

Nairn's book is a valuable contribution to the literature on technical assistance and international development. It examines the experience of UNESCO in two of its main projects in Thailand: the Pilot Project at *Cha Cheong Sa* which was intended to demonstrate new educational methods for subsequent diffusion throughout the school system, and the Thai UNESCO Fundamental Education Center (TUFEC) at Ubon, designed as a training program for community development teams. A brief description, for comparative purposes, is added of training programs organized by automobile distributors, a German technical institute, and mission schools. Bi-national programs conducted by the United States, and projects of other international agencies, are not examined, although it is asserted that their records are no better than those of UNESCO.

Given the sharp limitations imposed by the scope of this book, despite its title, Nairn has provided a sharply etched case study of failure, failure to design and carry out an effective program which would in fact diffuse new technologies and attitudes or even establish a viable institution. The eventual demise of both projects testifies to their failure. This catalog of inadequacies in program conceptualization and implementation should be required reading in any training program for project leaders engaged in technical assistance activities.

For political scientists, the book holds less interest. On the administrative side, it points up some hard lessons on the conduct of field operations by an international agency. Reading between the lines, one gains insight into the nature of intra-bureaucratic politics in Thailand. One of the more successful endeavors sponsored by the Pilot Project, for example, involved setting up, in response to the stimulus of a resident agent from the Food and Agriculture Organization (FAO), of a demonstration village. The program lasted for only a year, however, and although Nairn says that the "reasons for its demise are obscure," he adds that "There was also friction between the Thai Ministry of Interior and the Ministry of Education. The former thought that the scheme was an unwarranted intrusion into its domain by another department which, administratively and functionally, should have had nothing to do with agriculture." (p. 54) The main lesson belatedly learned by UNESCO in Thailand appears to have been that it could only assure success by entrenching its projects under the protection of a powerful sponsoring arm of the bureaucracy, notably the Ministry of Interior and its field apparatus of governors and district officers.

A more far-reaching inference drawn by Nairn from his studies involves a challenge to the whole concept of foreign aid which he regards as "interference in a nation's lifeways. In reality," he continues, "it is as much a continuum of the technological confrontation, despite variations, as the implanting of Western techniques on Asian societies by colonial powers." (p. 190) For Nairn, it seems, the basic issue is the desire of people in a traditional society to maintain a culture and style of living suited to their needs, values, and habits as against the pressure of foreigners, whether under colonial rule or voluntary assistance programs, to introduce technological changes which are inherently disruptive.

In this perspective Nairn fails to raise more far-reaching questions. Is it feasible for countries with rapidly rising populations, spurred by new medical and public health programs to endure the decline in living standards which must assuredly result if traditional technologies persist unchanged? Can societies with urbanized cosmopolitan governing elites sustain traditional agricultural modes of life? If the answer is no, then the real issue is not whether to engage in developmental activities, but how to minimize costs while maximizing benefits of such operations.

Among the conditions which, politically speaking, obstruct development in Thailand there is one that Nairn mentions only tangentially. He writes, ". . . the general ecology of the nation has as yet not given rise to other [extra-bureaucratic] groups with sufficient strength to compete for the fruits of power or indeed greatly influence policy or decision-making." (p. 95) Yet, in my judgement, it is precisely the concentration of political power and decision-making in the bureaucracy, military and political, that makes the intra-bureaucratic struggle for power so acute and tends to stifle projects which require inter-agency cooperation.

This point is well understood by David Wilson who writes, "The tendency of the bureaucracy to break up into competing groups because it is under no pressure from articulate expressions of interest from constituencies outside the government results in a dispersal of power to make policy." (p. 163). Wilson's book on Thai politics remains today the most convenient survey of the subject. The 1966 reprint in paper-back form makes it more accessible, but not more up-to-date. A new preface adds this useful comment: "the problem of assimilating a parliament to the system of bureaucratic dominance is the salient, ideological, and institutional question of Thai politics. While it has been possible to postpone a confrontation of this issue, the problem remains." (p. vi) Unfortunately, however, Wilson passes up the opportunity to up-date the contents of his volume or to revise its conceptual scheme in the light of materials published since 1962.

For example, an important point on which further clarification might be provided involves the structure of cliques, ruling groups, and *coups d'etat*. In a polity where neither the elected assembly nor the hereditary monarch exercise much power, the decisive political arena is intra-bureaucratic. Power is held by those bureaucrats capable of seizing it, and rival cliques consisting chiefly of military officers with their civil servant supporters, play the main role in this arena.

Wilson treats the Thai clique as a relatively autonomous informal organization, oriented primarily to its leader. He then gives biographical information about half a dozen of the chief Thai leaders thought to be the heads of cliques. Later, under the head of political organization, he describes the "khana" as a cluster of cliques, though it is not clear from his discussion whether a khana is the conspiratorial group that plots to seize power or the ruling group which dominates a government after the coup. Elsewhere he refers to a "ruling clique" as one that has seized power. (p. 158) He then refers to a "shift of power" or *coup d'etat* as equivalent concepts, thereby equating the transfer of power from one clique or khana to another with the use of violence. Undoubtedly violence has typically been used, or threatened, when one ruling group has replaced another, but not always, as Wilson's discussion of the 1944 shift of power demonstrates. Moreover, there have been military *coups* which caused minor readjustments and realignments in the ruling group, but not a major shift of power. In other words, the shift of power and a military act are normally associated, but not synonymous events.

A better analysis of the clique structure would have shed light on these phenomena. I believe clique structures are best analyzed as anthropologists do segmentary societies, not primarily in terms of their leaders. In these terms a khana is a cluster of cliques formed to seize power. Those khana members who subsequently hold cabinet positions constitute a ruling circle or coalition. The prime minister or coalition leader may, but is typically not, a clique leader—more often he is chosen by clique leaders to serve as a front man for the coalition. Hence although some, but not all, of the leaders described by Wilson headed cliques, the leaders of other cliques are neglected by Wilson because they did not become well-known or take the post of prime minister. In these terms, also, the relativity of clique structure can best be understood, for cliques are segments of a ruling coalition, and if one clique succeeds in displacing its rivals, the component factions of which it is composed tend, in their turn, to become cliques.

Failure to appreciate the dynamics of this process leads Wilson, for example, to exaggerate the importance of Phahon, who was a front man for the first ruling circle, and to neglect Song, leader of the senior clique in that circle. The result is a misunderstanding of the events of 1933 in which Phahon is described as leading the group which seized power from Mano. (p. 119) More accurately, both Mano and Phahon were "fronts" during an inter-clique conflict in which the senior clique, under Song, first tried to undermine the junior clique by driving Pridi out of the cabinet, with Mano's connivance (April 1933), and then Phibun drove Song and the senior clique out of the government, with the connivance of Phahon (June 1933).

These events are not, of course, intrinsically significant, but they established a pattern of inter-clique conflict and a political style which has, with variations, persisted until now. It is important not only for the study of Thai politics, but because similar forms of bureaucratic politics are to be found in many—though not all—of the non-Western States, and because they help explain many of the problems so clearly described in Nairn's book. Since Wilson's book contains much valuable and perceptive information about the history and dynamics of Thai politics, it is to be regretted that he failed to make use of the opportunity, in bringing out a new edition, to revise his discussion so as to make it an even more accurate reflection of the political system. Perhaps this is a service he can yet perform in a future, revised, edition of his important book.—Fred W. Riggs, (Riggs) *University of Hawaii.*

Reminiscences of the Cuban Revolutionary War. By Che Guevara. (New York: Grove Press, 1968. Pp. 287. $6.95.)

Venceremos! The Speeches and Writings of Che Guevara. Edited by John Gerassi. (New York: The Macmillan Company, 1968. Pp. 442. $7.95.)

In eulogizing Che Guevara, Fidel Castro called him "an extraordinary man," and urged all Cubans "to be like Che." To students of revolutions and of the people who make them, Guevara must also have a special place; for Che not only completed the revolutionary cycle—moving from the hills of the Oriente to the power of Havana—but like the winning gambler who dares to throw the dice one more time, Che went to Bolivia. Only this time the hills led not to the capital, but to his death.

Although these two books do not provide us with much insight into Che's decision to enter Bolivia, they do span critical phases in the Cuban Revolution and perhaps in Guevara's own development as a revolutionary leader. The first, *Reminiscences of the Cuban Revolutionary War,* is written by Guevara and tells of his experiences in the Oriente from the landing of the small guerrilla force up to Castro's final drive on Havana. Based on notes he took in the field, it contains terse descriptions of the small battles and difficult marches undertaken by the column he led. Che's style is often matter-of-fact and the thoughts and emotions of Che and his comrades are usually left to the imagination of the reader. Yet Che does provide some good vignettes of his fellow guerrillas, and his description of the march to Santa Clara and the ensuing battle is quite exciting, as is his account of the trial of a guerrilla who had killed a comrade while holding him under guard pending disciplinary action. To be sure, Che is no Hemingway, but he does give us some idea of what it must have beem like to be there, and we do learn something of the preoccupations of the guerrilla leader—of the need for salt, the concern for safety, the problems of morale and discipline and the sheer physical ordeal of initially untrained and inexperienced men fighting and living in rough country.

Venceremos!, a collection of the speeches and writings Che produced after the fall of Batista, shows a Che with a much broader perspective than that of a guerrilla. Here we find him interpreting the events and the meaning of the revolution; lecturing and writing on Marxism, Socialism, Capitalism and economic theory; and above all, calling for a new socialist morality—one in which human fulfillment is realized through the identification with a new, prideful Cuba. While these selections are often repetitive, polemic, and heavy with crude propaganda, they do reveal a good deal about the appeals used by the regime and could be an important source for research on the operational code of Che and his colleagues.

The introduction provided by Gerassi, who also edited and compiled the volume, presents a biographical sketch of Che which is based in part on interviews with those who knew Che as well as on Gerassi's own talks with Guevara. Although a bit too admiring, it does offer useful background material. It does not, however, give us much of an explanation for Che's revolutionary mentality other than to say that he was tough and courageous, and had become concerned about the poor and the oppressed in Latin America as a result of a trip he took throughout Latin America as a young man. Perhaps other students of Che will one day provide us with a more penetrating analysis—on which might, for example, examine the possible effects on Che of such factors as the severe asthma he contracted as a child, the separation of his parents, and his business failure in Argentina. It would also be interesting to know more about Che's relationship with Castro—a relationship which has long puzzled students of the Cuban Revolution.

In any case, Guevara emerges from both of these books as a complicated and many-sided man. Like his revolution, he would seem to be grappling with a series of conflicts. When he speaks of the privations of the peasant, of malnutrition and unemployment, and of industrialization as the way to a better standard of living, his stance is that of a humanist. Yet the same man also reports ordering the execution of comrades who had attempted to withdraw from the guerrilla band, or who had otherwise strayed from the proper revolutionary path. The justification? That the revolution at that point was not strong enough to imprison them, only to kill them.

These selections also show Guevara's implicit handling of other issues, such as Cuban nationalism vs. Cuban dependency on the U.S.S.R.; the need for a mass movement vs. the need for a revolutionary elite; and the need for a bureaucracy to organize and administer the revolution vs. Che's fear that it engendered red tape and inefficiency, and threatened to separate the people from the revolution. And finally, there is Che's attempt to mate Communist ideology with the Cuban experience—an issue he chose to resolve by arguing that Marx bears the same relationship to the Cuban Revolution that Newton does to physics. Marxism, in short, is a basic scientific finding as well as an orienting concept—one which must be creatively applied to a particular revolutionary setting.

Throughout Che's speeches and writings he stresses the need for pragmatism, yet he spends a great deal of time trying to interpret the Cuban Revolution—to build a theory, it would seem, from events whose meaning he may only partially have understood. And here, of course, is the irony: that the principal theoretician of the Cuban revolutionary experience, the one who professed to know its lessons, should be the one to blunder into Bolivia. Perhaps for all his analysis, Che finally yielded to his barely repressed romanticism, and like Garibaldi felt compelled to charge forth once again to do battle—rejecting, as did Garibaldi, hard-won participation in government. Or perhaps poor Che felt the need to be purged of his success—for "revolution," said Che, "purifies man."

In any event, while these two books tell us much about the man, they fail to tell us why Che missed an essential fact of the Cuban Revolution: that it did not succeed because it was communist but because Castro appeared to be a non-communist revolutionary—a position that made the Cuban army reluctant to fight and resulted in the defection of the middle-class from Batista. In short, Che not only misunderstood the political environment in Bolivia, but he failed to understand that the ideology which had been used to consolidate the revolution in Cuba would be a liability when it came to exporting revolution in Latin America.—WILLIAM H. BRILL, *Georgetown University*.

Communications and Change in the Developing Countries. EDITED BY DANIEL LERNER AND WILBUR SCHRAMM. (Honolulu: East-West Center Press, 1967. Pp. 333 $9.50.)

This book stems from an international conference on communications and the developing areas held at the East-West Center in Honlulu during the summer of 1964. It contains theoretical papers and case studies about India, China, and the Philippines. The results suggest that communications theory is lagging behind the fresh insights being gained in field experiences. Lerner and Schramm attempt to provide an overview, and they are quite perceptive about the role of the communications specialist: he must gain a warm intuitive insight into traditional society and then present the villager with modernizing innovations in an attractive light. They are rather unhelpful about situations of deep-set conflicts between the two "worlds" of tradition and modernity, and the danger that enhanced communications can contribute to that conflict.

Wilbur Schramm provides the keynote to the book in his introductory chapter, which asks how communications can be a tool of economic development programs. He emphasizes the need for a specialist to have a "fit to the culture" (p. 21), but is unclear on what this implies for the role of communications in the larger society. If as he contends, "the really basic strategies of developmental communication . . . are economic and political" (p. 27), who is to decide the political orientation of the specialist? Won't his contact with peasants change his approach? How can he lobby for his new-found insights in the seats of power? A Pakistani fellow at the Center, Inayatullah, hints at these questions in a *cri de coeur* against the ethnocentrism of foreigners and the Westernized native elites. He contends that they ignore the historical innovations and emotional supports of traditional culture (chap. 7). Daniel Lerner simply responds with an outspoken defense of the "Western model." He deplores the false dichotomy of "spiritual" East and "material" West, denounces Asian ethnocentricity against minority groups, and apparently contrasts it with the "self-denying ordinance" of the U.S. not to use its long-term development aid for short-term political purposes (p. 107).

He is even more direct in the conclusion. Traditional society is closed, inertial, and undercuts a man's ability to empathize with the unknown and venturesome. His recommendations follow quite logically: communications can help to reverse the "revolution of rising frustrations" by teaching men to be empathetic. The "policy sciences of democratic development," focusing on "system sustenance" (p. 310), can give firm advice to the policy-maker, and he praises the Socratic philosopher-king ideal.

Lerner's model is quite attractive if one assumes that the villagers can accept incremental amounts of information without perceiving them as attacks upon a rewarding and coherent style of life. He apparently assumes so, and this helps to explain his strange insensitivity to the advantages of "ineffectual" ideology (p. 311) in offering the villager a new sense of life's coherence, however wicked the consequences for his later acceptance of democratic values. His model also seems to assume that the larger political order has resolved its major conflicts and is devoted to development activities.

The other articles in the book do not confirm the optimism about the straightforward virtues of enhanced communications levels, and suggest that the traditional order can teach us a thing or two about empathy. Lucian Pye sensitively analyzes the role of the press in the developing areas (chap. 3). He suggests that the journalist's norms demand that he play inconsistent roles, that of being "inspector-general" of the government's activities, but also an agent of the nation's development program. Better communications can help weak social institutions to build consensus, but effective guidelines still must be worked out through painful experience. The economist Harry Oshima persuasively argues for the central importance of media in publicizing the results of selective growth investments (chap. 5), but Myron Weiner implies that the effectiveness of communications is largely dependent on economic and social variables and cannot be examined in isolation; for example, even as India's press became more influential it helped to exacerbate communal tensions through its sensationalism. India's S. C. Dube further suggests that almost nothing of assurance is known about the impact of communications on traditional society (pp. 95–96). His pessimism is reinforced by Schramm and Ruggels (chap. 4). Their use of 1961 multinational statistics fail to confirm Lerner's 1958 hypotheses on the importance of urbanization for literacy and media growth (in his *The Passing of Traditional Society*). They discern different patterns of media growth in various areas of the world, rather than single world-wide similarities.

Another article by S. C. Dube uses a number of survey studies to stress the importance of mouth-to-mouth communication in India, the most impressive being the amazingly fast spread of oral news about the 1962 Chinese invasion. C. R. Nair then praises the integrity and freedom of the Indian press, but also criticizes its small attention to development. Frederick Yu offers the startling contrast of the verve and intricate organization of Communist China's propaganda efforts to develop the "socialist man" through pageantry and indoctrination (chap. 12). Robert Worth follows with an historical account of the Chinese communists' skillful interweaving of modern and folk medicine in the villages after 1955.

Lawrence Fuchs recounts the experiences of early Peace Corp volunteers in the Philippines. They found that their "communications role" as village school teachers was largely a process of self-discovery about the limitations of American life (chap. 15). Any lasting progress required that they sacrifice a good deal of achievement-orientation to adapt to a more "personalistic," pleasure-centered Philippine attitude. The important point is not that they learned how to manipulate villagers out of their "unempathetic" narrowness, but had to recognize their own lack of empathy and pace their goals accordingly. G. D. Feliciano and T. G. Flores' perceptive and hilarious case study outlines the attempts to introduce sanitary toilets in the nation's rural areas (chap. 16). Development planners gradually had to adapt their designs to the villagers' own experiences and anxieties.

The volume as a whole suggests a disquieting future for the "policy science" of communications. Lyndon Johnson's Foreword and Lerner's idealistic model ring with the consensus attitudes of 1964, and thus have a certain nostalgic charm. However, they force us to ask if communications theory must inherently neglect a questioning of development goals on the one side and the virtues of tradition on the other. The central issue is not who is ethnocentric, but how to get cross-cultural feedback to the highest policy levels. We are still far from that goal, but the first step must be to recognize that the traditional man's defensiveness can stem out of his legitimate fear of losing something treasured and satisfying.—David J. Butler, *The Johns Hopkins University.*

France in the Age of the Scientific State. By Robert Gilpin. (Princeton: Princeton University Press, 1968. Pp. 474. $12.50.)

With this book, Robert Gilpin has come very close to establishing the definitive model for a case study of science policy in an industrial nation. Whatever reservations one may hold about science policy *per se,* Gilpin's effort has begun to achieve both the scope and the precision that will ultimately provide firm intellectual foundations for the study of interaction among science, technology, and public affairs.

Its chief strength is that this is not a book about politics alone. Rather, it is a successful merger of economics and sociology with the study of policy and politics. The result is coherent and methodical, yet unmarred by superficiality or jargon. If there is a drawback to this approach, it is the blandness that creeps into summary remarks on the force of tradition, values, national character, and institutions. Yet there is little doubt that behaviour patterns and ideology inculcated during the nineteenth and early twentieth centuries have been (and continue to be) crucial barriers to the process of modernization. The difficulty is to delineate them sharply and with economy.

The structure of the book is reasonably clear, though there is some overlapping. Each of four groups of chapters focuses on a different aspect of the underlying issue: what is the role of the state in an era of rapid change, in which scientific technology provides the main motive force. The first three chapters elaborate this question in a framework conditioned (if not determined) by the dynamics of American development and its repercussions in trans-Atlantic economic, financial, and political relations. Chapters four through nine provide a detailed review of the historical forces in French education, politics, and public administration, leading up to the present situation. This view is thrown into relief by a short chapter interpreting the American experience as a kind of ideal type.

The next two chapters—ten and eleven—are devoted to a careful examination of French science policy in two key areas—institutional modernization, and the expansion and politization of the scientific and technical community. In the final two chapters the contemporary dilemma of France —and by extension of Western Europe and other middle-sized industrial states—is analyzed in relation to the centripetal attractions of the American model, the option of a common "European" solution, and the vague attractions of independence.

Because of the general excellence of this study, one may cavil only at small failings. On the conceptual side, it is not made clear whether the proposed "imperatives" for scientific and technological advance and exploitation are modeled on the United States as it is, or as it would like to be (or ought to be). In short, one senses that a number of normative assumptions are not adequately examined, on the theory that success provides its own justification. Does this suggest that the French, and other nations, should strive to emulate and reproduce, analyze and select, or absorb and surpass the American experience?

On the methodological side, there is some evidence that budgetary data has been rather uncritically reproduced from official French sources. While it is useful to have so much information available in one place, it would be helpful to have it presented in a more comprehensible fashion. For example, this reviewer would have preferred reading budget amounts in percentages as well as in gross sums. A dollar conversion has become a standard device in publications of international organizations, and might have been adopted here.

Obviously a great deal of translation was necessary, and most of it, while not elegant, is workmanlike. However, some readers may be uncomfortable in the presence of renderings such as "law-program" (legislative program), "in effect" (in short), and "supple" (flexible), which are less misleading than literal and awkward.

A study so ambitious and detailed partially satisfies every specialist craving, and excites new appetites as well. This reviewer would have liked to find a discussion of tax measures for the promotion of private industrial research, for example. The somewhat rhetorical treatment of the relation of economic planning to governmental research investment may leave students of economic policy hungering for more practical information. Likewise, the scientific community is not analyzed as an emergent interest group, though an adequate foundation is laid. In all these cases, however, the notes are copious and suggestive.

One especially intriguing gap is the influence of that generation of scientific adviser-administrators who admired the Soviet model and who were at one time members of the Communist party. Only Frédéric Joliot-Curie is mentioned more than briefly, yet the political activities of this scientist and his colleagues commenced between the wars and continued until the emergence of the Fifth Republic.

The internal and external politics of change has many aspects, of which the mobilization and application of science for social purposes is of great contemporary interest. Gilpin's study of one national society moves us away from the monographic, disciplinary, and culturally limited stage of analysis without sacrificing the flavor and importance of national patterns and traditional disciplines as sources of insight. What remains in question is the objectivity and utility of a scheme that is Atlantic in scope and tends to merge Europe's fears with America's dreams.—T. Dixon Long, *Case Western Reserve University.*

Korea and the Politics of Imperialism, 1876–1910. By C. I. Eugene Kim and Han-kyo Kim. (Berkeley: University of California Press, 1968. Pp. 260. $7.00.)

The study of the decline and fall of any kingdom or dynasty is always a fascinating subject to a serious scholar. Compared to the voluminous studies on the fall of the Ch'ing dynasty or the coming of the Meiji restoration, the demise of the Yi dynasty in Korea has seldom been studied by a disciplined scholar. There are some studies on the Japanese policy to seize Korea: H. F. Conroy's study in the United States and C. C. Mun's study in Korea. Studies on some phases of the fall have appeared recently: K. J. Shin's study done in Japan and published in Korea and Yi Na-yŏng's study in North Korea in 1965. But there has been no monograph, until this study, that covered the three decades of the intricate story of the opening and annexation of Korea.

This book, by two Associate Professors of Political Science at Western Michigan University and the University of Cincinnati, is valuable, not merely because the authors have done the difficult work of recounting complex forces that operated in the process of the Korean demise, but because they have shown a remarkable understanding of history and an amazing perception of the imperialistic designs of foreign powers that operated in the fall of the Yi dynasty. The result is a pithy and highly commendable inquiry into the circumstances leading up to the Japanese annexation of Korea.

The book in four parts covers the fall from the first treaties concluded to the final annexation treaty of 1910. It begins with succinct but excellent introductory notes on traditional Korean political life, and unfolds the events which attest to the manifest and growing Japanese influence at the expense of Chinese suzerainty over Korea. The Un'yō incident that resulted in the first treaties, the Im'o mutiny of soldiers, the political imbroglio of Kapsin, and the Treaty of Tientsin all are examined. The precarious domination of Korea by China prior to the Sino-Japanese confrontation, the indigenous rebellion, and the Russo-Japanese war that successfully eliminated the last of the rivals for Japan are closely scrutinized. The cautious and consistent Japanese advance that ultimately incorporated Korea into Japanese empire is also evaluated. The authors concluded that Korea's inability to transform its society to meet change, persistence of factionalism, lack of popular participation, lack of resilience and adaptability in Korea's political system, and rapid growth of Japanese power, all contributed to its fall.

The study is basically sound and the accounts of intricate workings of the imperial forces are well presented with uncommon objectivity. But the study suffers from the lack in general of critical analysis of the events so well documented. Often it is merely an account of events without any meaningful evaluation of the evidence presented. At times it is only a summary rather than analysis of events. There is analysis only of the reform movement of 1898 (pp. 112–14); such analysis was needed in each part at least, if not in each chapter.

Some five or more pages of data, for example, were introduced (p. 154) with little or no analysis of the Japanese control measures. A case was also made to stress that only three percent of the populace were participating in the decision-making process (several tables appended to the book), but how this fact and the tables are related to the fall of the dynasty was not explained. Particularly in Part IV, there are prolonged discussions on the role of Bethel, what seems to be the minor if not irrelevant role of missionaries, and the exaggerated role of the Ŭibyŏng; but there is little analysis of how these affected the Japanese use of coercion in Korea. A convincing argument has been advanced as to the lack of resilience and adaptability of the traditional Korean political system, but there is relatively little discussion on the Korean political system itself. Compared to a relatively detailed account of the workings of imperial powers, there is little account of the roles of Koreans. This is perhaps a reflection of predominantly Japanese sources used by the authors.

The authors presented well the passing of the old and coming of the new subjugated Korea. They have skillfully described many facets of the fall of Korea; some similar to that of China and others peculiar to Korea. The encroachment of imperialists to open a secluded and reluctant hermit nation that was incapable of reform and plagued with internal rebellion is very similar to the Chinese experience. The actual breakdown of the old tributary system to give way to the new treaty system was exemplified by the Korean demise. The authors correctly pointed out that it was indeed an Aisan imperialist that ended and engulfed the Korean kingdom. The defeat of Russia and China's desperate but unsuccessful clinging to the fiction of "the autonomous but dependent" Korea was also unique to the Korean demise.

The book provides a much needed study of the period, and is a valuable addition to the growing studies on Korean politics in the United States. The authors' research for the detailed facts is meticulous, and they adhered scrupulously to a comprehensive conceptual framework. They have written well and with erudition the account of the demise of the Korean kingdom.—DAE-SOOK SUH, *University of Houston.*

The Whig Party and the French Revolution. BY F. O'GORMAN. (New York: St. Martin's Press, 1967. Pp. 270. $9.00.)

Donald Southgate has pointed out that while Dr. Johnson thought the Devil must have been the first Whig, Lord Acton favoured the claims to that honour of St. Thomas Aquinas. Certainly for much of the long period from 1688 to 1885 there was room to accommodate no less strange bedfellows beneath the protective cover of Whiggery. Yet there were all the while changes and recombinations of philosophical and political forces taking place which eventuated in the emergence of the 'new' Whigs as an identifiable force in the modern British party system. For most of the 18th century, of course, as the painstaking researches of the late Sir Louis Namier and his disciples established, there was no 'party' system at all, but rather an elaborate system of factional politics based on family and patronage, in which the Whigs were more or less all those who opposed the Crown and Administration group in the parliament. Since Namier, interest has been high in tracing the steps by which party grew up at Westminster. We now have the major work of Sir Keith Feiling on the Tories, while on the Whig side, there have appeared in addition to a number of excellent monographs on prominent Whig statesmen, important books by such writers as Brooke, Foord, Derry, Black and Pole. What is clear, the writer of the study under review believes, is that as our knowledge of party development deepens, simple structural interpretations of the growth of party, dependent on one dominant variable such as family tie or patronage system, will not suffice. "Belief in a principle for its own sake, prejudice, fear, ignorance and hesitation," O'Gorman suggests, must be taken into account alongside the more tangible factors. He has arrived at this conclusion after a close reconstruction of shifting political allegiances in 'old' Whig circles during the crisis years of 1789–1794, when the old alliance of Whig factions was shattered by the historic decision of the Portland Whigs and the 'Third Party' forces of Windham and Burke to join in supporting the war government of the younger Pitt. The Foxites were thereafter left virtually alone in opposition, and from there formed the vehicle through which the 'new' Whigs of the 19th century received the ideological legacy of "Peace, Economy and Reform."

The general features of Whig politics in these years are well-known, and need no elaboration here. O'Gorman finds structural, ideological, and chance elements which contributed to the breakdown of the old Whig machine. The appearance of political clubs, the growing reliance on party newspapers and party symbols for mobilizing support and extending party identification, the remarkable contributions to party financial organization of William Adam—all these posed a threat to the continued domination of the Whigs by family notables. While the Foxites supported such new vehicles of public opinion as the radical Association of the Friends of the People, and many of them favoured parliamentary reform, the issue of the extent to which the party should adopt a popularizing stance became a major one separating the progressive and traditionalist element in the party.

So far as the ideological factor is concerned, it was the events following in the train of the French Revolution, as O'Gorman argues in the main section of his narrative, that after 1793 irreversibly tipped the scales away from the old balance of forces. From the beginning Fox saw in the Revolution "one of the most glorious events in the history of mankind," while first the government benches, then aristocratic Whigs, and finally public opinion swung behind Burke's vehement denunciations of French Jacobinism. In the end, the issue became a struggle over the definition of party that ended in disaster for the Whigs. The Foxites strained to portray their differences in foreign policy as a "particular issue," not fundamental to the Whig attachment itself. Yet the great theoretician of party government in 1770, Edmund Burke (of whom Goldsmith had jibed: ". . . born for the universe, narrowed his mind, / And to Party gave up what was meant for mankind."), was now in his *Observations on the Conduct of the Minority* (1793) above all else the advocate of "joint endeavours [for] the national interest." There was no inconsistency with the notion of coalition government in Burke's view of party, and as O'Gorman observes, a "delighted" Edmund Burke looked on as Portland accepted the seals of office under Pitt.

Finally, there were elements that accelerated the separation traceable simply to personality and lost opportunity. While the Duke of Portland, wishing to be all things to all men, stood as titular head of the party, it was the uncontrollable Fox, passionate defender of great causes but curiously lacking in political sensitivity, who held the hearts of the younger members. Burke was as high-minded as Windham was vacillating, and others like Loughborough were more concerned for "personal honour." And the lost opportunities! We are told that during the entire three weeks leading up to the fateful debate on the Quebec Bill, when Burke's intentions to break openly with Fox were widely known, little was done by any faction to reconcile the two.

This is a well researched and heavily documented study that will be welcomed by those interested in British party history. It is not always easy going, and assumes some acquaintance with the background events of the period. But it is reliable, and will be a required reference for more ambitious interpretations of the general development of party politics in the historical and comparative perspective. Dr. O'Gorman, in several appendices, provides important estimates of the relative strength of the Whig factions at Westminster during the period.—GEORGE FEAVER, *Emory University*.

Revolutionary Russia. ED. BY RICHARD PIPES. (Cambridge, Mass.: Harvard University Press, 1968. Pp. 365. $7.95.)

The fiftieth anniversary of the bolshevik revolution provided the stimulus for many conferences and books, of quite varied quality. The papers presented at the Harvard conference on this theme are of more than passing interest; together they form a useful statement on the present state of Western research on the Russian revolution. Although the conference organizers made no effort to provide a chronological history—these already exist in abundance—the inclusion of many of the West's most distinguished students of the revolution assured a broad representation of current research interests and interpretive efforts. These interests range from the old and continuing questions, such as the reasons for the breakdown of the tsarist autocracy, and the role of German intelligence in the pre-revolutionary period, to a restatement of the well-trod ground of the October events in Petrograd; to such relatively unexplored areas as the political thought of the First Provisional Government and the Petrograd Soviet, the aspirations of Russian society in the March days, and the conflicts between the town and country in the civil war. Three interpretive essays—by E. H. Carr, Maximilien Rubel, and Adam B. Ulam—are directed to the often raised but finally irresolvable question of the "meaning" of the revolution. In addition to topical variety the essays, and particularly the formal "comments" and informal discussion following each paper reflect the great ideological and methodological differences among students of the revolution. Far from detracting from the book's value this heterogeneity enormously enhances its utility. The evident conflicts force the reader to adopt a critical attitude to the premises, evidence and conclusions of each contributor.

While it would be presumptuous to attempt criticism of each article—the comments and discussion following each essay point well enough to their more obvious shortcomings—some general observations may be in order. In a penetrating, lucid essay Mr. Kennan reviews the evidence once again on the chances for the survival of the Russian monarchy had not the war intervened. Optimistic at the outset, Mr. Kennan finds that neither the slowness of the regime to grant political reforms, nor its excessive nationalism, nor the personal limitations of the Tsar—all critical factors in the final fall of the monarchy, in his view—could be blamed on the war. Neither were these factors substantially on the way to correction in 1914. Therefore, he concludes, there was no real hope for the dynasty's survival. In my opinion, Mr. Kennan's essay is an example of the best in interpretive, analytic history. In raising broad questions, he never strays far from factual data in seeking their answers. This same can be said of Adam Ulam's highly insightful reflections on the gap between practice and ideology in the revolution and how both Lenin and Stalin justified this to themselves. Unfortunately, one does not get the same sense of a careful reverence for factual support behind the interpretive efforts of Maximilien Rubel. His thesis that the Russian revolution was not a socialist but actually a capitalist revolution led by the *de facto* Russian bourgeoise rests upon a thin semantic argument and requires alteration of long established and accepted notions of capitalism as having something to do with private ownership of capital and a market exchange system operating under more or less competitive conditions. This as well as other fundamental methodological shortcomings in Mr. Rubel's essay are well formulated in the comments by Shlomo Avineri.

Mr. Richard Pipes has presented us with the finest piece of straight historical research and expository writing in this collection. His thorough study of the sparce documentation on Lenin's early political activities and thought combine with his closely reasoned argument to make a most valuable contribution to "Leninology." Of particular interest is his discussion of Lenin's "conversion" to Social Democracy in the summer of 1895.

The question of the role of German intelligence in Russia prior to the revolution has always been the center of fierce ideological conflicts among Western scholars. Those who would like to believe that among the Bolsheviks were many "paid agents" of the Germans tend to be less critical in their evaluation of the existing scanty evidence of German-Bolshevik connections, while those opposed to this view for ideological reasons refuse to credit anything but "legal proof." George Katkov's paper represents the former position; the critique by Alexander Dallin the latter. If one evaluates Mr. Katkov's essay solely as a report of the available evidence of German political intervention, without considering the broader conclusions he draws from it, then it must be recognized as a solid piece of research. Yet, an important test of scholarship, in my view, is the care with which conclusions and assertions are related to evidence. In this sense, Mr. Dallin's acid remarks are well taken—Mr. Katkov's limited data permit only very limited conclusions. Together, Mr. Katkov's and Mr. Dallin's contributions show the extent to which ideological positions influence the research of Western scholars.

The analyses of the political ideology of the First Provisional Government and of the leaders of the Petrograd Soviet by Mr. Schapiro and Mr. Anweiler respectively deal quite competently with complex and important questions. Their arguments and conclusions elucidate effectively the reasons why these two bodies both failed to work together and failed to provide effective leadership in a revolutionary situation.

From the point of view of a political scientist, perhaps the most interesting article in this collection is Marc Ferro's study, "The Aspirations of Russian Society." Its significance lies in its methodology and the type of data utilized. Ferro attempts to assess popular attitudes through statistical and textual analysis of letters to the editor received by *Pravda* and *Izvestia* in the first few weeks following the fall of the Monarchy. His method is similar to that used in several studies of the American revolution. Yet, to my knowledge, this is a first in Soviet studies. While Mr. Ferro's results so far, presented in this collection, are rather sketchy and somewhat disappointing, the important point here is the possibilities for future research this method may open up. While both the author and Mr. Thompson in his comments on Ferro point out the extreme caution with which the results of such studies must be interpreted, the experiences gained by Mr. Ferro and the Ecole Practique des Hautes Etudes in gathering and processing this type of data may suggest and make possible more fruitful studies of the role of public opinion in Soviet politics.

On the whole this is a valuable collection of interpretive and analytic essays on the general subject of revolutionary Russia. Both the student and general reader will find it well worth reading.—Philip D. Stewart, *Ohio State University*.

Soviet Leaders. Ed. by George W. Simmonds. (New York: Thomas Y. Crowell, 1967. Pp. 405. $10.00.)

This collection of biographical sketches of 42 prominent figures in Soviet government, science, and the arts provides interesting biographical reading and a very useful reference work for both the interested layman and the political scientist. Since it was compiled primarily with the former audience in mind (p. v), the book contains a glossary of political and historical terms (e.g., collective farm, Council of Ministers of the USSR, Executive Committee, *kulak*, etc.). It is usefully indexed by name and subject. The 42 essays are written by 20 scholars, some of whom are well known to American students of Soviet politics John Hazard, Grey Hodnett, Robert Slusser, and Severyn Bialer) and others who are probably not as well known (R. Milner-Gulland, Maxim Mikulak, and Siegfried Müller-Markus). The essays vary in length considerably, from Kermit McKenzie's biography of Anastas Mikoyan (23 pp.) to

Severyn Bialer's biography of Konstantin Vershinin (3 pp.).

The editor's Introduction to the book presents a brief chronology (20 pp.) of events in the Soviet Union since the death of Stalin. In the first half of the essay Khrushchev's changes in domestic and foreign policy are contrasted to the earlier period of Stalinism; in the latter half Khrushchev's policies are compared to those of his successors. There is little of value here for the reader who has at least a general knowledge of Soviet politics, be he political scientist or layman.

The value of the 42 biographical sketches for the interested layman or the non-specialist is clearly more notable. Aside from official Soviet sources (almost all of which are in Russian), the only detailed sources of biographic data on Soviet personalities are the directories published from the biographic data in the files of the Institute for the Study of the USSR in Munich, Germany: *Biographic Directory of the USSR* (1958); *Who's Who in the USSR: 1960/1961* (1962); and *Who's Who in the USSR: 1965/1966* (1966). These three directories merely list demographic and career positions (with dates). The volume under review is in the form of readable prose and has the added advantage (for those who desire or require it) of placing this information in the perspective of the times and discussing, with the information that is available, the personalities and achievements of the subjects. The editor claims that the contributors "have avoided a partisan approach" in this endeavor (p. v), and that claim seems a wholly accurate representation. However, this more elaborated form of biography contains instances of interpretation (or embellishment) of data which one might question, such as the conclusion that "Semichastny is . . . probably less intelligent . . . than Shelepin . . ." (p. 86).

Because of the availability of the Institute biographical publications, it is rather strange that more use was not made of them by the contributors to this volume. Only five of the biographers (Slusser, McKenzie, Hodnett, Parry, and DeGeorge) included them in their bibliographies. Presumably, they were the only five of the 20 who made use of them in compiling biographic data for their essays. This is especially strange in that all but four of the 42 subjects (Liberman, Neizvestny, Solzhenitsyn, and Voznesensky) are listed in the second edition of the *Who's Who in the USSR*. In several cases where the Institute publications were not utilized, the biographic sketches in this book did not include some relevant material contained in the former. Especially is this so in the case of several of Bialer's essays. Of course, the validity of the Institute's data files is questioned by some. If there is good reason to question their validity, then the five contributors who utilized the Institute's publications should neither have used them nor have suggested that their readers consult them. On the other hand, if those files are considered reliable, the other contributors should have made use of them.

This prompts one to consider the many problems which confront the researcher in his quest for reliable biographic data on personalities in all Communist systems, especially the Soviet Union. Since the purpose of this volume is to present such data, the reader is entitled to some explanation concerning the methods employed and the sources utilized. In this case, it would appear to be the responsibility of the editor to inform the reader on these matters. Yet nowhere in this volume are questions related to the validity of data and the reliability of sources discussed. In a paper read at the 1963 Annual Meeting of the APSA, Severyn Bialer demonstrated his keen awareness of the problems of reliability in the collection of Soviet elite biographic data. One can assume, therefore, that he exercised extreme caution in the utilization of official Soviet sources. One would hope that the other contributors exercised similar caution in dealing with problems such as the systematic distortion of social background data so that it appears that most, if not all, prominent figures came from industrial proletarian or peasant families. Since there is no discussion of the question of source reliability in this work, we have no way of knowing what, if any, consistent standards were used in the compilation of the biographic data.

The 42 personalities selected for inclusion in this book are well-chosen, given the aim of presenting "a composite portrait of the current Soviet elite" (p. v). In the case of the party, government, military, and foreign service officials in the book, they clearly occupy the top positions on the various organizational charts. However, one must be aware of the fact that this selection places the most severe limitations on the generalizability of the "composite portrait." Unfortunately, no caveat to that effect appears in the book. That is a significant omission since the book is directed mainly at a lay audience and especially because the similarities in the lives of these elite members are emphasized. A caution against over-generalization or stereotyping is not merely appropriate, but mandatory, in this country where a good many people think that all Russian women look more or less like Nina Khrushchev.—FREDERIC J. FLERON, JR., *University of Kentucky.*

La socialisation politique des enfants. BY CHARLES ROIG AND F. BILLON-GRAND. (Paris: Armand Colin, 1968. Pp. 186.)

La socialisation politique des enfants is the first published report which extensively discusses the political socialization of French school children. The data are based upon questionnaires administered in 1963 to 413 children betwee the ages of ten and fourteen living in or near Grenoble. Although the authors could not study all areas of the city (most importantly, they could not gain access to the schools in the Red Belt), they did question students in both Roman Catholic and state schools. Thus they not only are able to present global findings, but also to discuss some interesting subcultural differences.

The most interesting findings concerned the absence of partisan attachment and the lack of positive affect for political authorities. Less than one-half answered yes when asked if there were any good political parties and only 13 per cent named a political party they thought was good. The U.N.R. was the most popular party, but it was supported by only 10 per cent of the children questioned. (Unfortunately, the authors did not gather data on children's perceptions of their parents' party identifications and do not comment on the well-known interpretation of lack of partisan support presented by Philip E. Converse and Georges Dupeux.)

The authors also find a relatively high level of distrust for public authorities. When asked to explain what the President of the Republic does, only eight per cent viewed him as a benevolent leader; less than one-half designated a politician or statesman as the man who has done the most for France; only one child out of five named a politician as a person he would like to resemble, whereas two out of five listed a politician as a person they would not like to resemble. Nearly all of the children could name De Gaulle as the President of the Republic, but when asked to list his good and bad qualities, there was a higher percentage of unfavorable than favorable responses. The authors argue that these unfavorable evaluations of the President reflect more of a resentment toward authority in general than a disapproval of De Gaulle and his policies.

The children do not appear anti-parliamentarian. Asked whether a country should be led by one man or by an assembly of deputies, three out of five preferred government by assembly. Only one out of five said a country should be governed by one man. Surprisingly, those who wanted a government by one man were more likely to believe there were good political parties than those who favored government by assembly. This is explained by the high percentage of U.N.R. supporters among those who thought there were good parties.

Roig and Billon-Grand are well informed about American social science and, in particular, by American findings about the political socialization process. David Easton, Robert Hess, and Fred Greenstein are among the persons most frequently cited. One of the most valuable contributions of this book—to French readers at least—will be an introduction to some recent American social science.

The American reader, however, is likely to be both disappointed and confused by the data analysis. Throughout, the authors present data in the form of two-way contingency tables. There are usually three breakdowns: by sex, by type of school (Catholic or state), and by class in school. Initially, the breakdown by class in school appears to present pseudolongitudinal patterns, but this classification involves more than age differences. The oldest children (in the end-of-studies class) are of heavily working class origins compared to the children in the middle two years. Also, children educated in church schools have higher social origins than those in state schools, and girls have higher social origins than boys. Thus to the extent that interesting subcultural differences do emerge, it is difficult to know whether they are related primarily to age, sex, type of school, or social class of origin. Also, and more serious, the authors sometimes select one item to measure an overall political orientation and then present two-way contingency tables showing the relationship between responses on that item and other attitudinal items. For example, they employ one item "Was the French Revolution of 1789 good or bad?", as a measure of Right-Left orientation and then show the relationship of responses about the Revolution to 24 other items. True, some interesting relationships emerge, but one cannot use such findings, as the authors do, to make broad generalizations about the French Right and Left.

But the American reader will learn much about French political culture from this book. One of the authors' most interesting arguments is that one should discard such preconceived notions as France being a conflictual society (as contrasted with an American consensual society). Rather, they argue, there are probably basic areas of consensus in France although, they admit, they do not find them in their data. Most importantly, Roig and Billon-Grand have demonstrated that socialization research can be conducted in French schools. Problems of access may be greater than in the United States, but they are not insurmountable—Paul R. Abramson, *Michigan State University.*

Thailand: Another Vietnam? By Daniel Wit. (New York: Charles Scribner's Sons, 1968. Pp. 205. $4.95 & $2.50.)

American involvement in Vietnam has caused many observers and statesmen to look at Thailand as a possible alternative base in the resistance against Communist expansion in Southeast Asia.

Thailand is the only relatively large unified non-Communist society located near Vietnam. It is the only nation in the region which did not fall under European colonial rule, and the absence of an anti-Western sentiment has encouraged it to cooperate actively with the American containment policy since 1950. This action has caused the Peking regime to mark Thailand as the next country for a "war of national liberation." Already several thousand Communist insurgents are waging guerrilla warfare in the northeastern provinces of the kingdom.

Thus the talk of "another Vietnam." The ominous title of Professor Wit's book is heightened on the cover of the paperback edition which shows a lush green map of Southeast Asia broken by a bright red undercoating beneath the torn fabric of Thailand. Yet only in brief sections at the beginning and near the end of the study does the author directly address the challenging issue of Thailand becoming "another Vietnam." It is very possible that the newsworthy title of this book and the sensationalized cover of the paperback edition were chosen by the publisher rather than the author.

The study provides much good data and analysis on which to judge the future of the Thai kingdom. It is a well written and scholarly piece of research. The book is well documented and includes a vast array of useful statistics on the Thai educational system, the economy, and the administration. It contains brief but good background information on the cultural and social forces that affect Thai politics.

The bulk of the study is an assessment of the Thai political system and the strengths and weaknesses of Thai society. The author's major thesis is that the Thai government for many centuries has provided stability and security to its people and it has gradually adjusted to new changes and new demands. Yet the ruling elite today must build a broader representative base if it is to cope with new economic and social challenges in the near future. More of the political system, Professor Wit claims, must be institutionalized and more politically oriented groups must be brought into the decision-making process. If these reforms are not forthcoming, Thailand will be subject to greater internal instability and the likelihood of more external infiltration.

The deficiencies in the study are relatively minor. In places the discourse is hampered by long outlines enumerating factual data and analytical commentary which the author feels is of special importance. This practice tends to make the discussion somewhat rigid, wooden, and oversimplified. Also, the author never resolves the dilemma posed by his assessment of the widespread devotion among the Thai people to Therevada Buddhism with its emphasis on suffering and detachment from worldly pursuits and the contagious and joyous enthusiasm of contemporary Thai society for rapid material progress.

Perhaps the most serious weakness to this reviewer is the heavy stress by the author on government planning and the need to create "pioneers" to guide the kingdom in its uncertain future. Professor Wit never elaborates on who these innovators will be and what role they will play in future *political* development. His proposal very likely involves a political contradiction. If the political reforms he desires are inaugurated, more groups and more leaders will be brought into the political life of the nation. Open debate and free political activity will cause more diverse opinions on economic, social, and international affairs. Some Thai will want to place much more of the national economy under public ownership. Many will propose more private enterprise. Some will advocate a neutral foreign policy. Many will want to retain close relations with the United States. This development will make it less rather than more likely that the government will be able to engage in any consistent and precise long-range economic and social planning. The price of democracy in Thailand as elsewhere will be a greater role for conflicting political groups and some political and administrative disorder.

In spite of these shortcomings this is an excellent book. The question posed in the title is never directly answered. Fortunately, the author provides much good information and analysis on which the reader can make up his own mind.—FRANK C. DARLING, *DePauw University.*

The Fate of the Revolution: Interpretations of Soviet History. BY WALTER LAQUEUR. (New York: The Macmillan Company, 1967. Pp. 216. $5.95.)

Since the historians of the French Revolution are still debating the shape of its events and their meaning almost two centuries after the *revolte nobilaire* opened the period of revolutions, it is not surprising that the Russian Revolution a half century later is still the subject of passionate partisan and scholarly controversy. *The Fate of the Revolution: Interpretations of Soviet History* is Walter Laqueur's contribution to the flood of books on the fiftieth anniversary of the revolutions of 1917. It is a reasonably dispassionate and scholarly attempt to sum up the problems faced and the positions taken by the more important historians and political scientists, by apologists like Barbusse, by official Soviet historians, by loyal oppositionist historians like Leon Trotsky and Isaac Deutscher, by semi-critical analysts of the accounts of the victorious like E. H. Carr, and such varied writers as Billington, Ulam, Gurian, Feiler, Arthur Rosenberg, Fainsod, Brzezinski, Borkenau, Leonard Schapiro, Bernard Pares, Bertrand Russell, Boris Souvarine, Prince Mirsky, George Kennan, and the present reviewer. Some of these he summarizes and analyzes in depth, others only tangentially. In the case of the former he attempts to establish the background of each historian's point of view, the problems he raises, and the level of his skill and scholarship. In each case he strives to present objectively the positions expounded before venturing or withholding his own comment. Though Mr. Laqueur has not read everything of significance he has come as near to it as any man writing on the occasion of the fiftieth anniversary of the fateful year 1917, including the major bulk of writings in Russian and English, and key works in German. Thus he has produced a combination of a critical bibliography with a good summary of major viewpoints raised during the entire half century of historiography, political analysis, and controversy, and of the problems debated and still subject to debate. Of particular interest today are his analyses of such questions as the applicability of the term totalitarian to the Russian one-party state aiming to embrace and control all the concern of Russian society; the uses and weaknesses of "Kremlinology"; the problem of whether Stalinism was historically "necessary"; and the soundness or unsoundness of the "convergence theory."

Mr. Laqueur's credentials for handling these difficult tasks include the former editorship of the British journal *Survey*, coeditorship of the *Journal of Contemporary History*, directorship of the British Institute of Contemporary History, and the post of Professor in the History of Ideas at Brandeis University in this country.

It might be well to close this review with the author's own statement of the scope and purposes of his book:

> I have not tried to break fresh ground as far as source material is concerned, nor have I attempted an historical synthesis. The aim was less ambitious. I wanted to provide a guide for the non-expert, to stimulate further study and critical thought rather than offer my own answers to the many questions posed. [The resultant book] is a study of the problems which have faced historians of the Soviet Union and a discussion of their interpretations . . . an introduction but neither a history nor a bibliography. . . . I have tried to reproduce or summarize fairly views and arguments that are not my own. . . .

The task he thus set himself is unique in historical writing on the Russian Revolution and is competently performed.—Bertram D. Wolfe, *Hoover Institution on War, Revolution, and Peace.*

The Rise and Fall of Kwame Nkrumah. By Henry L. Bretton. (New York: Frederick A. Praeger, 1966. Pp. 232. $5.95.)

In this critical reappraisal of Ghanaian politics Professor Bretton argues that Kwame Nkrumah subsumed all Ghanaian political organizations within a tightly controlled personal machine. In Bretton's view, Nkrumah did not need to share decision-making or balance contending groups, as previously supposed, because Ghana lacked any organized social forces or ideas other than those dependent upon its leader. Scholars have neglected this system of personal rule, Bretton asserts, due to acceptance of the institutional label for the fact and a morally irresponsible tendency to condone autocracy. Betton strives to correct these failings, moral and intellectual, by getting behind the forms to the "real actors." The result is a curiously unreal apotheosis of Nkrumah consequent upon presenting his extensive powers abstracted from their setting and embellished with Cold War hyperbole.

This formalism does not arise from disregarding backstage manipulation but from excessive concentration on "top-level control of the power and influence structure." This viewpoint excludes the overall process of decision-making and focuses narrowly on Nkrumah's often unused final authority. Hence, Bretton does not treat institutional continuity as a reflection of decisions shared by organizational leadership, nor describe the common organizational practice of weighting decisional alternatives by pre-emptive acts, nor assess the political resistance to innovation manifest in unfulfilled commands. The omission of a decisional framework, descriptive or conceptual, facilitates these exclusions, as does the absence of a case study of Nkrumah's exercise of power.

Moreover, the view from on high reduces Ghana's actual cultural and social fragmentation to an atomistic void; Bretton treats the political machine as disassociated individuals linked only by Nkrumah and rejects even systems analysis as imposing false coherence on the data. To establish the "cultural wasteland" Bretton concentrates on the lack of a unitary national culture and coherent national doctrine. He ignores such influential belief systems as traditional attitudes towards authority and ethnicity, the English Liberal sub-culture of the educated sector, the emerging African Socialist sentiments of conflict avoidance and communalism, Christianity and Islam. In his assessment of political doctrine Bretton confines the significance of Nkrumah's ideas to the tactical flexibility of empty abstractions. Yet both Nkrumah's supporters and opponents readily identify him with such substantive ideas as African unity, the threat of neocolonialism, the economic role of the state and the centrality of politics.

Unlike Ghanaians, Bretton ascribes no political importance to economic, status, ethnic, or geographic conflicts. He regards the organizations which reflect these strains as cogs in the machine.

The role of the Central Committee of the Convention People's Party in decision-making has not been established; but it can not be assessed—as Bretton attempts—by accepting without qualification the post-coup pleas of Party leaders that they were only following orders. While the Party had only a minor role in mass mobilization after independence, Bretton excludes the Party's role in linking groups of stalwarts, assimilating opposition blocs and recruiting new partisans. Bretton also minimizes the significance of other organizations both as repositories of power and mechanisms of control. His extensive survey of machine building omits the political struggles required and mistakenly treats Nkrumah as the sole beneficiary of power shifts. He exaggerates the stifling of parliamentary debates and underestimates civil service resistance. He ignores the strongest Party Wing, the Farmer's Council, which effectively challenged Government policy.

The residual system of personal rule failed, Bretton contends, due to its incapacity in four functional areas: it lacked the ability to learn and correct, sacrificed stabilizing institutions to the machine, mis-directed the economy and compromised the aim of ideologically inspiring leadership. The regime actually possessed relatively good information and certainly corrected some of its serious errors respecting agriculture and political disruption of the civil service and local government. Nkrumah did not destroy the stablizing framework; the continuing state institutions—army, police and civil service—destroyed him. Nkrumah did contribute to economic disruption and failed to bring about an ideological transformation. A serious assessment of these weaknesses would, however, require comparison with both Nkrumah's military successors and other African regimes.

Bretton prefers a broader canvas: he seeks to assess the "age-old pattern of personal rule" against the "whole spectrum of alternatives developed by mankind over a period of 4,000 years." History, rather than establishing conditions or limits, offers a smorgasbord of possibilities. Unencumbered by the decisional process, systems repercussions, culture, society or history, Nkrumah emerges in unpredicated freedom to decide a nation's destiny and only himself to blame in defeat. The fable may be a salutary reminder of the responsibilities of leadership, though I am unpersuaded of the rectitude of a judgment which systematically depreciates situational factors. It does not accurately portray the Ghanaian political process.—Ernst Benjamin, *Wayne State University.*

Philippine Collaboration in World War II. By David Joel Steinberg. (Ann Arbor: The University of Michigan Press, 1967. Pp. 235. $7.50.)

The military occupation of the Philippines by the Japanese during World War II resulted in a long-lasting collaboration crisis for the Filipinos in their relationships with the United States and Japan. The Philippines were approaching independence in 1941 and had been relatively successful in blending Western and Eastern cultures. After the Japanese invasion and occupation, the necessity for survival of the Filipino people forced a degree of compliance with the new military government. The author notes that ". . . not everyone can go into exile or join a guerrilla band, and yet those left in occupied territory have the greatest need to preserve the sanctity of their national ideals." David Steinberg, a University of Michigan history professor, has researched very ably the actions of the Philippine elite or the establishment as they struggled with the countless conflicts that arose between 1941 and 1948.

The Filipinos, led by President Quezon strongly backed the United States in 1941. Although the United States had planned full military defense of the Philippines, the Pearl Harbor disaster caused a major change in the defense perimeter of the allied forces and left no hope that the United States would retake the Philippines in the immediate future. A major objective of the Japanese after establishing military control of the islands was to create a pro-Japanese government and a pro-Oriental society. Steinberg traces the steps taken by the Japanese to woo the Filipinos. An executive commission was appointed, comprised of well-known Filipinos who had been known to be in varying degrees sympathetic to the Japanese. The author traces the reasons for the selection of Jorge Vargas, executive secretary for Quezon, as head of the commission rather than Begnigno Aquino or Jose Laurel.

The Japanese recognized the necessity of matching the American plan for independence for the Philippines. Japan set the date for 1943 and the first preference for President was Manuel Roxas, who had been second in command in the Quezon administration. After much reflection on the part of the Japanese, they named Laurel to head the new Republic. Laurel and the Japanese were unable to alleviate severe food shortages and inflation *was so rapid* that the country was faced with an economic collapse. Laurel attempted to accommodate the guerrillas by amnesty declarations but was never able to eliminate the vague excitement of MacArthur's promised return. The nadir or climax of the Laurel administration came with the declaration of war against the United States in September, 1944.

After the American invasion of Leyte in October, 1944, Sergio Osmeña was named by the allied forces to head a pro-American Philippine government. The author describes the influence of MacArthur in securing the freedom of Roxas and explains now MacArthur played a key role in Roxas' 1946 election victory over Osmeña. Osmeña had been urged by the Americans to purge all Filipinos who held office under the Japanese occupation but he held firm to a policy of judging each case on its merits.

Steinberg has a penetrating analysis of the fate of the Filipino leaders who collaborated with the Japanese. The Philippine Peoples Court filed 5,603 cases of political collaboration but only 156 were convicted. The author selected 165 of the most influential Filipinos arrested by the Americans immediately after reconquest and compared their cases with the People's Court records. Of those arrested by the Americans, 122 had no record in the Peoples Court. Eleven, including Laurel and Vargas, had their cases dismissed or were not prosecuted. Only Teofilo Sison was convicted and his was the initial case. Thus, the author concludes, ". . . the elite remained intact. . . . The war diminished neither the size nor the authority of the establishment."—H. ODELL WALDBY, *Florida State University.*

The Citizen and the Administrator in a Developing Democracy: An Empirical Study in Delhi State, India, 1964. BY SAMUEL J. ELDERSVELD, V. JAGANNADHAM, and A. P. BARNABAS. (Glenview, Ill.: Scott, Foresman and Company, 1968. Pp. 170. $6.00.)

Eldersveld, et al., argue that economic development in India requires that the public understand and support government officials. Thus, they attempt to study the network of interactions between a highly differentiated set of officials and the heterogeneous publics they serve, "with the particular goal of determining the relevance of bureaucratic contact for citizen perceptions, attitudes, and orientations." (p. 7) The book is a conscious replication of Janowitz's 1954 study of public perceptions of Detroit officials and illustrates the current effort to extend scientific analysis to the study of politics in non-Western areas. At the same time, the authors' mistakes and omissions are indicative of the problems which such attempts have encountered. I will comment only on some of the substantive questions raised by their findings.

Underlying the argument of the book are a set of assumptions about what constitutes "modern" administration, and thus what the goals of administrative development should be. For example, the authors assert that "the basic test, then, for political development in our research is the relative strength and direction of the societal environmental factors and individual predispositions, on one hand, and the stimulus resulting from bureaucratic contacts, on the other hand." (p. 13) Eldersveld and his associates never discuss why stimuli from the bureaucracy rather than social and personal factors within the electorate should be the distinguishing feature of "modern" political processes.

The most interesting and provocative findings in the book raise questions about the nature and effects of urban life in India (or Delhi). These findings are scattered (pp. 20, 24–8, 31–2, 40, 42–3, 46–7, 49, 136–7) and never discussed as a whole. The findings indicate that rural areas are more supportive (i.e., less critical or "hostile") of officials and government than are urban dwellers and that they also have more contact with officials than do people in Delhi. Though the mean level of contact is low, in the urban sample criticism of administrators increases with contact. Why is it that the rural citizens are better informed, score higher on sense of efficacy, manifest more self-confidence in dealing with officials, and have more contact with officials than do city dwellers? Given greater contact and sophistication, why should rural citizens be more supportive of officials whereas increasing contact is correlated with greater hostility in the urban sample? Considering the lower response rate in the city (64% vs. 85% for rural) and the unexpectedly high rate of "don't know" responses, can these anomalous findings be artifacts of a biased urban (and/or rural) sample? If they are indeed valid findings, must we then re-evaluate our assumptions about the integrating and modernizing effects of urbanization when dealing with India?

This apparent paradox may stem from the authors' failure to clarify their own interpretation of respondents' criticisms of administrators. Sometimes they equate criticism with cynicism or disaffection, as when they label it "hostility." Elsewhere they question whether non-critical responses among rural groups (especially illiterates) really do indicate a genuine support of the government, thereby suggesting that criticism may be a reflection of "realism" (their term) in dealing with administrators and even of integration into the modern community. Considering the high levels of contact, efficacy, self-confidence, and knowledge in rural areas, one might guess that the proportion of "realistic" and integrative criticism is higher there than in urban areas. But why should this be so? One can only regret that Eldersveld and his colleagues have not gone more deeply into this aspect of the analysis, but have contented themselves with presenting largely descriptive material.

The interpretability of the data is also diminished by a perpetual vagueness about procedures. There is a suggestion, for example, that only men were sampled, but some women were interviewed (response rate 96%). It is not clear whether these women were included in the analysis. Likewise, the exact scoring of indices is never indicated, though usually the items in each index are mentioned. Castes are ranked into high, middle, and low, but with no indication of the specific castes in each category. Since the tables report no middle castes in the city, the Vaisya castes (who as traders, businessmen, and artisans constitute a large proportion of all urban areas) must be included in either the high or low cells, but no justification for this decision is given. Furthermore, the number of respondents is rarely and irregularly included in tables. In some places the authors mention that a particular finding is or is not significant, but nowhere is it stated whether this evaluation refers to statistical significance or to theoretical and practical importance. Finally, one wonders how well respondents understood some of the questions and how willingly they responded. Administrators may have been both confused and threatened when asked, "What per cent of the public do you think would say that people in your position are efficient?"

This review has focused on the defects more than the strengths of Eldersveld's efforts because these defects prohibit a full assessment of the data (which are quite interesting), and because the value of a "pilot project" depends heavily on its explicitness as a guide for future research. While some of the defects are due to faulty research design, others may be due simply to carelessness or to poor judgment on the part of a copy editor. Whatever the causes, one hopes that Prof. Eldersveld and and his associates will remedy these deficiencies in the reporting of his current survey efforts in India. There is no reason why survey research in non-Western areas should not meet the standards of rigor and precision expected of such work in the United States.—David J. Elkins, *University of California, Berkeley.*

Politics and Change in a Traditional Society: Lebanon, 1711–1845. By Iliya F. Harik. (Princeton: Princeton University Press, 1968. Pp. 324. $9.00.)

Students of political culture and change, whether or not they have a professional interest in the Middle East, will find Iliya Harik's study of the decay of the feudal order in Mount Lebanon illuminating. Harik has provided us with a careful analysis based on scrupulous documentation, and his historical discussion is always pertinent to his model of political change. The political dynamics of Mount Lebanon's crumbling feudal order are complex, but Harik is admirably concise in his exposition, and the reader not familiar with the area will be grateful for the clarity of his interpretations, chapter summaries, and conclusions.

The Imarah of Mount Lebanon from the mid-eighteenth to mid-nineteenth centuries is described as an iqta', or feudalistic, political system ". . . in which authority is distributed among a number of hereditary aristocratic chiefs subordinate in certain respects to a common overlord." (p. 37) Harik describes the political dynamics of this system, which was headed by the Ma'ni and Shihabi dynasties, in terms of a systems model whose three components are the legitimacy principle, institutionalized practices, and actors. The appearance of new and contradictory principles, practices, and actors causes a disruption of the system. Harik uses the model to discover the political loads that arose in the Imarah, no small feat given the complexity of its traditional pluralism and the fragmentary documentation available. In the stable iqta' order the legitimacy of the Hakim (Ruler) arose primarily out of the ancient class divisions of the Mountain, and authority was institutionalized in a gradated yet reciprocal relationship between the Hakim and the muqati'jis (local hereditary chiefs and collectors of the iqta'). The common people did not constitute an independent political factor. Sectarianism, one of the main characteristics of contemporary Lebanon, had no place in the iqta' order.

Why did this sophisticated system finally break down in civil war, sectarian strife and foreign intervention? In an analysis which must be commended to all interested in processes of political change, Harik shows how the institutional conflict between authority and kinship fragmented the power and enhanced disintegrative tendencies among the muqati'jis, and how the conflict between hereditary and electoral practices aggravated the problem of succession of the Hakim. On the systemic level a more serious disintegrative force was the rise of the Maronite community to political significance. Harik skillfully traces the origins of communal feeling among the Maronites and describes how the Maronite clergy and peasantry arose to challenge the legitimacy, institutions, and main actors in the iqta' order. He identifies the clergy as a principal source of political organization, innovation, and disruption, and he shows how it infiltrated the ruling structures, for example, through the administrative office of the mudabbir. It also politicized the common people and precipitated the popular uprisings of 1820 and the 1840's. Harik's masterly portrayal of the rise and fall of the Maronite mudabbir, Jirjus Baz to mention one of several examples, focuses sharply upon the conflict between the established and the innovating forces. He shows why the innovating forces, represented by the Maronites, were unable to perpetuate their newly-established position, how the Druze muqati'jis unwittingly undermined their own power position through excessive reliance on the Ottoman government, and how the last of the great Shihab Hakims, Amir Bashir II, maneuvered in vain to maintain a broad basis of legitimacy.

Inevitably, some points might have been clarified. It would be helpful to have further explanation as to why the Shihabis became Maronite. Further attention might have been devoted to the reasons why a feudal spirit still persists in Lebanon, even among Maronites, despite its attrition by the new sectarianism and its official abolition by foreign powers. Evidence of the Lebanese entity notwithstanding, one still might question the intensity of national feeling among the people: thus, speaking of the qa'immaqamiyyah, is it perhaps an exaggeration to say that "(t)he problem in this partition arrangement was that the Lebbanese were not really willing to divide their country"? (p. 35) Compared to the particularistic relationships of the iqta' system, te emergence of a communal Maronite feeling may be called universalistic, but the latter designation seems inappropriate when viewed in terms of the confessionalism in the modern Lebanese republic. In the early nineteenth century communalism was, for better or worse, a modernizing force: today, however, in Lebanon and elsewhere in the Middle East the communal order, along with the still important remnants of feudalism, it would seem to be an obstacle to satisfactory political performance.

This study is not only a valuable contribution to our understanding of political culture and integration in Lebanon and the Middle East, it is also an informative case study of the effects of differential political development within a political community. The model is shown to be workable, fruitful, and relevant to modern political theory; as such, it deserves application elsewhere.—MICHAEL C. HUDSON, *Brooklyn College of the City University of New York.*

African Opposition in South Africa: The Failure of Passive Resistance. BY EDWARD FEIT. (Stanford: The Hoover Institution on War, Revolution, and Peace, 1967. Pp. 223. $7.50.)

This work by Edward Feit, currently Assistant Professor of Government at the University of Massachusetts, is a revision of the author's Ph.D. dissertation at the University of Michigan entitled *Conflict and Communication: An Analysis of the "Western Areas" and "Bantu-Education" Campaigns of the African National Congress of South Africa Based on Communication and Conflict Theories.* It also draws upon his earlier monograph, *South Africa: The Dynamics of the African National Congress,* which was issued under the auspices of the Institute of Race Relations in London and published by Oxford University Press in 1962.

African Opposition in South Africa . . . is an incisive and dispassionate analysis of the strengths, weaknesses, and strategies of the oldest African nationalist organization in South Africa, the African National Congress, which dates back to 1912. Although, as the title of the doctoral dissertation suggests, it draws heavily from communications and conflict theories, it also utilizes both structural-functional and decision-making modes of political analysis. Such a behavioral approach to the increasingly arcane and complex field of ethnic politics and polities in the Republic of South Africa is all the more refreshing due to the generally barren wasteland of South African studies which has far too long been inundated with a surfeit of descriptive and/or polemical writing. Professor Feit's book is, without question, a significant contribution to South African studies in particular and to African studies in general because its research design is germane both to South Africa and to other countries and regions of Africa, giving it both comparative and heuristic utility.

Professor Feit's study of the African National Congress draws heavily from the records of the Treason Trial in South Africa as well as from *The Bantu World,* an African newspaper published in Johannesburg, which chronicles the events of interest to the urban African population of South Africa and which, to a considerable degree, reflects the attitudes of the African Third Estate towards the programs and personalities of the African National Congress. The Treason Trial records, which have by no means been fully exhausted by those engaged in research on South Africa, are a case of serendipity in terms of source material on the structure and function of the African National Congress. By utilizing these records, which have been catalogued by Professor Thomas G. Karis (in *The Treason Trial in South Africa: A Guide to the Microfilm Record of the Trial* [Stanford, California: The Hoover Institution on War, Revolution, and Peace, 1965]), Professor Feit has limned an unusually revealing portrait of this preeminent African political organization.

In terms of the subtitle of his book, Dr. Feit suggests that the failure of passive resistance against the South African Government in the campaigns against the removal of African shantytowns in Johannesburg (the so-called Western Areas) to newer townships in the peri-urban areas where the Africans no longer enjoy freehold tenure and against an allegedly servile educational curriculum (the so-called Bantu education) stemmed not just from the power differential between the white body politic and Congress but also from the dysstructural organization of Congress. Moreover, the failure was attributable to strategic myopia on the part of the leadership cadre of Congress which, as the author demonstrates, was a byproduct of the asynchronous structure of the African National Congress. One of the most crippling maladies of Congress was the lack of feedback, owing to the sclerotic nature of the communications and command channels, and this, in turn, meant that there was a debilitating polycentrism within the decision-making structure. The dialogue between the leadership and the functionaries, on the one hand, and the leadership and its African clientele, on the other, was confused, discontinuous, and disjointed, with the result that there was considerable "noise" in the communications network. Closely related to the failures in organizational feedback were complementary failures in monitoring the activities and utterances of the subalterns by the elected national leaders.

In terms of strategies, Professor Feit cogently contends that Congress dissipated its rather limited resources in waging too many campaigns simultaneously rather than canalizing its efforts into one well planned and ably executed foray against the Government. Despite its self-appointed role as a mobilizing agent for the African majority, Congress nevertheless appealed to only a tiny fraction of the African masses; and, in addition, its penchant for coalescing with other ethnic groups in anti-Government campaigns alienated no small number of its articulate and politicized members who espoused a uni-racial, not a multi-racial, doctrine of Africa for the Africans. The decision-making elite of Congress was, as Dr. Feit has suggested, attuned to bourgeois values which placed a high premium on respectability which, in turn, fettered the leadership and caused them to intone the moderate idiom of non-violence and racial cooperation and parity, an idiom which increasingly alienated the militants. Here Dr. Feit's findings closely parallel those of Michels with respect to the social distance between leaders and clients, on the one hand, and leaders and their protagonists in parliament, on the other, although the command structure of Congress was such that it seemed to invalidate his iron law of oligarchy thesis.

The main flaws in Professor Feit's work are essentially minor and do not detract from the integrity of his research design or the importance of his findings. First, the concluding chapter was much too brief (a mere four pages); second, he made far too little use of the South African Hansards (and on the one occasion when he did—on pp. 148-149—he quoted not from the primary source itself but from a secondary source) and treated the M.P.s and Senators in Parliament who represented Africans at that time in far too cursory a fashion; third, he neglected to utilize one of the standard works on Congress (Mary Benson, *The African Patriots: The Story of the African National Congress of South Africa* [London: Faber and Faber, 1963]); and finally, he could with profit have been less parsimonious in his use of footnotes.—Richard Dale, *Southern Illinois University.*

The Politics of Violence: Revolution in the Modern World. By Carl Leiben and Karl M. Schmitt. (Englewood Cliffs: Prentice-Hall, 1968. Pp. 244. $5.95.)

In recent years many competent case studies and a few comparative analyses of political violence have been written, along with a small flood of essays on conceptual and theoretical issues in revolutionary studies, hortatory texts on the making of revolution, and establishmentarian treatises on their prophylaxis. James C. Davies, Chalmers Johnson, Nicholas Timasheff, and others have proposed new theoretical interpretations of revolutionary causation and process. However not since the publication of Crane Brinton's *Anatomy of Revolution* and George Pettee's *The Process of Revolution* three decades ago has anyone attempted a general, descriptively-comparative summary of what we know, or think we know, about revolution. This study by Professors Leiden and Schmitt is such an attempt, a remarkably good one. The authors have written the book primarily for students. In doing so they have provided a lucid, jargon-free, and reasonably comprehensive summary that provides both introduction to the subject and a bench-mark for further inquiry.

The book does not break new ground. It does not offer a new conceptual framework, propose a synthesizing theory, or report new comparative studies. In the authors' words it offers "a summary of the literature on the theory and analysis of revolutionary causation and development," to which they contribute modestly with brief studies of the origins and consequences of the Mexican, Turkish, Egyptian, and Cuban revolutions. The definitional question is dealt with first. The authors' reluctance to innovate is demonstrated by their reliance on Calvert's definition of revolutions as "forcible interventions, either to replace governments, or to change the processes of government." Elsewhere they distinguish several forms of revolutionary action: gradually-intensifying conflicts among large and powerful groups that, if unresolved, lead to civil or revolutionary war; coups d'etat; and mass uprisings and guerrilla activity, whose participants are largely outside the power structure. Unsuccessful attempts at revolution as well as successful ones are considered. A wide range of illustrative and theoretical material is used throughout. Most major and many other revolutionary movements of the twentieth century receive at least passing discussion; the classic revolutions get somewhat more than equal time. Lesser acts of political violence like riots and assassinations are seldom considered except when they occur in the context of revolution. There is for example no reference to racial turmoil or advocacy of revolution in the United States.

Much of the recent and older literature on revolution is summarized under conventional rubrics: The Prerevolutionary Environment, Stages of Revolution, Revolutionary Leaders and Followers, the Ideology of Revolution. The past 30 years have provided many new illustrations of the conventional catalog of underlying conditions that dispose men to revolutionary violence: foreign control, economic privation, demands for political participation, deterioration of elites and crumbling regime authority. But the only conceptual improvements represented here are new labels like "dysfunction" for these conditions, and the recognition that it is the deterioration of circumstances relative to men's expectations rather than absolute deprivation that frustrates men to the point of revolutionary violence. Evidence on the stages of revolution suggests—implicitly, not explicitly in this study—the inadequacy of Brinton's fever metaphor for conceptualizing the revolutionary process. The potential impact of foreign support and opposition on the genesis and outcome of political violence, made strikingly evident by post-war events, are recognized.

A number of myths about the nature of revolutionary actors are examined in the light of much evidence and then reinterred. Revolutionary leaders tend to come from the middle levels of their societies. Though some are fanatical and perhaps a few psychopaths, most are frustrated men of conviction, ambition, and great energy, not deviants in a clinical or criminal sense. They seldom if ever create revolts by themselves. Rather they mobilize followers from among their frustrated fellow citizens. *Lumpenmenschen* and peasants are said to be seldom among those followers; the upper segments of the working classes, the middle classes, and students form the backbone of most revolutionary movements. It is unfortunate that the authors, in making these judgments, give little attention to the conditions under which there has been mass mobilization of rural people by revolutionary movements, for example in China, Vietnam, Kenya, and the Congo.

A principle contention of the authors about revolutionary and related nationalist ideology is that much of it is designed *post hoc* and *ad hoc* to provide justification for revolutionary action and guidance for social transformation once revolutionaries have attained power. Neither they nor recent research provide much insight into the less formal justificatory ideas about political violence that circulate in prerevolutionary periods, or the relative importance of cultural attitudes toward violence, foreign ideologies, and indigenous revolutionary appeals for transforming discontent into political violence.

The four case studies that comprise the last part of the book are especially useful because for three of them—Mexico, Turkey, and Egypt—extant studies are partial and specialized rather than comprehensive. The case studies are too summary for many scholarly purposes but provide good illustrations of some of the generalizations developed in preceding chapters. The 300-item bibliography is occasionally idiosyncratic but more comprehensive than any other that is generally available.

Despite the extensive speculative and empirical material examined in the study it has certain lacunae. One is the relative inattention, dictated by the authors' regional interests, to revolutionary movements in contemporary Africa and Asia. Another is an unaccounted failure to incorporate the results of the systematic quantative studies made of political violence, either recent ones like those of the Feierabends and Tanter or older ones like that of Sorokin. The study can also be criticized for its failure to contribute to analytic clarity or theoretical synthesis, but to so criticize it would be to misconstrue its purposes. It is a wide-ranging and intelligible summary of the current state of knowledge, no more and no less. One of its principal lessons may be that such clarity and synthesis are still badly needed.—TED GURR, *Princeton University*.

The Origins of Communism in Turkey. BY GEORGE S. HARRIS. (Stanford: The Hoover Institution on War, Revolution and Peace, 1967. Pp. 215.)

It is surprising that the relations between the Kemalist regime of Turkey and the Bolsheviks in Russia during the early 1920's have not received more attention. In origin, context, and purpose, there were many parallels. Both regimes emerged from a long series of conspiracies dedicated to the overthrow of corrupt government and the reform of their respective societies. In both cases, success came in the wake of overwhelming defeat in the First World War. Both groups were faced with stiff opposition by the victorious World War I allies supported by local factions, including in the Turkish case the legitimate government in Istanbul. There was then a definite basis for a community of interest between the two revolutionary regimes. The means for implementing such a community of interest were also available, since the two governments had a common frontier in the Caucasus, well beyond the reach of Allied military or political power.

On the surface at least, Kemalists and Bolsheviks acted in accordance with their apparently common interests. They concluded a formal treaty of friendship early in 1921, marking the first full formal recognition of the Kemalist government by a foreign regime. The Kemalists, who were militarily hard pressed by Greek forces advancing across western Anatolia, also received substantial material assistance from the Bolsheviks. Friendly relations continued throughout the inter-war period, although significantly Atatürk made certain that his tight grip on the Turkish political system was not undermined in any way by Moscow-oriented communist organizations.

Indeed, it has been generally assumed that there was no Communist activity worthy of note in Turkey from the 1920's at least to 1960. There were political controversies in the late 1940's which involved charges of crypto-Communism and fellow travelling. These charges resulted in the dismissal of a number of professors from the University of Ankara, the resignation of the Minister of Education, and the end of the rather unique and highly interesting Village Institute program which had been designed to train primary school teachers and community development workers for the villages. This episode bore all the marks of a type of McCarthyism, however, and was probably politically motivated.

More recently, and particularly since the military coup d'etat of May 27, 1960, Marxist and other leftist ideologies have openly emerged on the stage of Turkish politics. The most concrete expression of these new trends has been in the form of the Turkish Workers Party. This group advocates extensive nationalization of business and industry, opposes foreign investment in the economy and American influences in particular, and favors a neutralist foreign policy. It has attracted widespread support among the intelligentsia but very little among workers and peasants, for whom it purports to speak. The question of identifying the political and intellectual antecedents of these latter day socialists calls attention once again to the gap in the literature on Turkish political history.

George Harris's interesting book is an attempt to fill this gap. It is a workmanlike history of the largely unsuccessful attempts to launch a communist movement in the confused and rapidly changing Turkish political environment between roughly 1900 and 1925. Harris is a close student of Turkish affairs, having spent about seven years in the country, five of them as an official in the American Embassy during the critical period of the late 1950's and early 1960's. The book reflects his familiarity with the subject. It provides valuable new insights into the turbulent formative period of the Turkish Republic.

Unfortunately, Harris limits himself to a straightforward account of chronological developments in the shadowy left wing of Turkish politics during this period. He therefore has missed the opportunity to comment on perhaps the most interesting contrast between Kemalism and Bolshevism. The Bolsheviks clearly represented a social and political upheaval of the most profound degree. Kemal's movement, on the other hand, was far more instrumental in its approach both to ideology and action. He was prepared to advocate revolution only in the political sense and only for the purpose of reestablishing Turkish politics on a stable foundation. Although he spoke of making the peasant master of the country, he never embraced any doctrine of total social reform or revolution. Moreover, while the revolutionary character of Bolshevism was evident from the first, the true nature of Kemalism did not begin to emerge until the Ankara regime was firmly entrenched in power. Indeed, it has been argued by contemporary leftist writers in Turkey that the Kemalists were socially conservative, and that the effect of their rule was to further entrench the traditional Ottoman elite. In the light of this analysis, the change of regime from Sultanate to Republic and of dominant ideology from Islam to republican nationalism were mere formal changes which failed to touch the basic social structure. Harris's account supports this analysis insofar as he documents Atatürk's consistent resistance to the social doctrines of the Marxists, even when he pursued a policy of cooperation with both domestic Communists and the Kremlin.

On the other hand, Harris's account gives concrete illustration of the attractive power of the Bolshevik Revolution for the underdeveloped nations. Time and again he shows how some of Kemal's supporters were disposed to look upon the Soviet regime as a model for Turkish development, and how easily Marxist ideas could be blended with the nationalism so typical of Turkey at the time and other countries of Asia and Africa since.

The book is occasionally marred by apparent slips of the pen. For example, on p. 99 there is a reference to "the present Turkish Communist Party." Since communism has been outlawed since 1925, it is not clear what is being referred to. Harris's conclusion to the effect that contemporary Turkish leaders have not succeeded in "incorporating . . . restless radicals into the system with the sure touch displayed by Atatürk a generation ago" leaves a great deal unsaid. Atatürk had an open mind to new and even radical ideas so long as his political domination was assured. Contemporary Turkish leaders, on the other hand, represent a social stratum that has traditionally been subordinate to the social and intellectual elite which spawned both Atatürk and the radicals who espouse Marxist ideas. To expect these new leaders to adopt the same expansive strategy towards ideological dissidents as the great national founder is therefore asking for a great deal. Indeed, this very fact speaks volumes about the social and political changes that have come about in Turkey and the grave problems that confront that country today.—FRANK TACHAU, *University of Illinois at Chicago Circle.*

The Congress Party of India: The Dynamics of One-Party Democracy. BY STANLEY A. KOCHANEK. (Princeton: Princeton University Press, 1968. Pp. 516. $13.50.)

Stanley Kochanek's study of the Indian National Congress is the third book-length study of the Congress to appear in the past four years. The other two, my own study of Congress party organization in Uttar Pradesh and Myron Weiner's book, *Party-Building in a New Nation,* were primarily concerned with the local, district organizations of the Congress and with the problems of building and maintaining strong party institutions in a modernizing society. The book under review has a different focus. It is a study of the relationships between party and government at the center and between the central and state units of the party in the period from 1946 to 1967, with an added long section on some aspects of leadership recruitment into the Congress.

The book is organized into three long sections—the first on the roles of and the relationships between the prime minister and the Congress president; the second on the executive institutions of the Congress, the Working Committee and its powerful sub-committees, the Parliamentary Board and the Central Election Committee; the third on Congress leadership. Also included is a postscript on the implications for the Congress organization of its loss of all-India hegemony in the aftermath of the 1967 elections. The book is based upon newspaper reports, Congress publications, personal interviews and questionnaires administered during two field trips to India, the private papers of three Congress leaders, and the minutes of the meetings of the Congress party in parliament. Some of this material is original and has not been used before by other scholars.

The organization of the study is largely historical. The history of the post-independence Congress organization is divided into three periods—a period of transition from 1946 to 1951, marked by conflict between the parliamentary and organizational wings of the party; a period of "centralization and convergence" from 1951 to 1963, marked by parliamentary dominance over the party organization and central dominance over the states; and a period of "divergence" from 1963 to 1967, marked by "equilibrium of power between party and government, center and states" (p. xxiv). The periodization is really nothing more than a statement of Nehru's personal role in party and government during his prime ministership, which, between 1951 and 1963, masked the great changes that were taking place in the Congress organizations in the states and which began to affect the functioning of central party institutions after 1963. It is unfortunate that this periodization is so frequently reiterated throughout the book and is used as very nearly the only organizational framework for the whole study, for it tends to obscure many of Kochanek's insights into the way the Congress system functions. For example, Kochanek does demonstrate that, through all three periods, the supremacy of the parliamentary wing on policy matters has been retained, but that party-government relations on matters of party and parliamentary power can become strained during periods of transition, both at the center and in the states. These points are worth making and they remain valid generally, irrespective of the periodization.

The periodization is also of limited use in analyzing central-state relationships in the Congress since the middle period is the longest and the one during which the most fundamental changes occurred in the character of the party organization in the provinces and in the relations between the provincial and central party organizations. Yet, in chapter ix, which deals with the Parliamentary Board and the issue of party-government relations in the states, two pages are devoted to the entire period from 1951 to 1963. More attention is devoted to this second period in other chapters, such as in chapter xi on the Central Election Committee, but here the applications of the periodization is not entirely convincing. Kochanek's argument is that the ability of the central leadership to intervene decisively in the candidate selection process has declined significantly since 1963 as a consequence of reduced unity among the national leaders of the party. This may be true, but Kochanek provides examples of effective central intervention from both periods—in the Punjab in 1962 and in Andhra in 1967.

Kochanek's analysis of party-government relations is also not satisfying. On p. 434, he points out that "the issue of party-government relations is more often than not a matter of intra-party factionalism in which the dialogue of competition for ultimate power in the parliamentary wing disguises itself as the problem of coordinating parliamentary and organizational wings." Yet, his history of party-government relations at the center in the first four chapters takes the dialogue for the reality. Thus, the great struggle of 1950–51, in which Purushottamdas Tandon became Congress president and was then forced to resign, to be replaced by Pandit Nehru himself, is interpreted as a grand conflict between party organization and the parliamentary party. In fact, it was probably less a conflict between Nehru and Tandon over party-government relations than a conflict between Tandon and Rafi Ahmad Kidwai, which had its roots in factional politics in Uttar Pradesh. The fact is that we have not yet had a full account of this turning-point in Congress history, which has elsewhere been interpreted as a conflict of ideologies, a conflict between Sardar Patel and Nehru, and is in Kochanek's study a conflict between party and government. All these versions are the top of the iceberg, the rhetoric and inside newspaper dope of Indian politics which cover complex interrelationships of personal ambitions and factional alliances extending throughout a subcontinent.

Kochanek has also failed to relate his analysis of party-government relations in the Congress to the vast theoretical and comparative literature on this issue. How do party-government relations in India in fact compare with party-government relations in Great Britain, from which the Indian theory has been drawn? Kochanek argues that, as in Great Britain, the parliamentary wing of the party has emerged supreme. Yet, the dynamics of the system are entirely different. For, the party organization is the road to parliamentary power in India and the parliamentary wing is supreme only when a single group or coalition in the Congress is united and controls both wings of the party. In other words, it is not tradition and precedent or a clear constitutional definition of functions which determines the pattern of party-government relations in India, but the relative balance of political forces in both wings of the party at a given time. Moreover, the issue of party-government relations has never been primarily an issue of policy responsibility, but rather it has been an issue of power in the party and in the government.

I believe that Kochanek understands these things and his book shows evidence of considerable insight into the workings of Congress politics in India. He has also provided some useful data on the changing and broadening social bases of the Congress at different levels of the party. What is lacking is a theoretical or analytical perspective sharper than a superficial periodization or a conventional analysis of party-government relations in an unconventional context. The book is also far too long and repetitious. The sub-title of the book is misleading and is especially inappropriate after the 1967 elections. India has never had a one-party system.—PAUL R. BRASS, *University of Washington.*

Party and People: A Study of Political Change in Tunisia. BY LARS RUDEBECK, (Stockholm: Almqvist & Wiksell, 1967. Pp. 275. SW. Kr. 25.)

Lars Rudebeck's *Party and People: A Study of Political Change in Tunisia,* is the latest of four major books on that country in English over the past four years, all of which are written by Western scholars. So far as I know, none of them has been translated into Arabic, the native language of Tunisia. This reviewer remembers with some concern how he searched in vain in Tunis' bookstores, during a visit in the summer of 1967, for books on Tunisia by Tunisians and finally settled for the only thing he could find—a biography of President Bourguiba put out by a government information office.

Faced with the disparity in scholarly contributions on development between the Western intellectual enterprise and that of the developing countries themselves, one's first impulse is to congratulate ourselves on our impressive record. However, before we rush to do so, it may be worth our while to ask if we are missing something of value by a failure to establish some measure of community of scholarship with the people we study. Lack of funds and pressing practical concerns may prevent intellectuals in developing countries from matching our record, but this should not make us believe that they do not exist. In studying developing areas, it is often felt, we listen to natives and talk to colleagues. It is going to take some time and thought, before we start learning how to talk to natives; and for a start we might cherish the hope of being treated some time to a review by a scholar from the same country we write about.

It perhaps adds to the interest in Rudebeck's book on Tunisia that it not only provides us with an excellent and informative account on the politics of that country but also illustrates important points about the state of our discipline. Consider the fact that Rudebeck is a Swedish scholar writing in English, with an American theoretical approach, about a North African country! Rudebeck has been influenced by the writings of David Easton and Gabriel Almond; indeed we may say that he has in this book followed Almond's scheme faithfully as it has been elaborated in *The Politics of Developing Areas.* The Almond scheme is very attractive and a too ready an acceptance of it may lead the researcher to gloss over the underpinnings of structural-functional theory upon which it has been based. Indeed, Rudebeck's book confirms the view sometimes held of Almond's version that it being so succinctly stated lends itself most easily to descriptive treatment of a political system. The author makes it clear, though, that not only is he concerned with describing how the party system functions in Tunisia but that underlying this is the thought that "an underdeveloped country today can be systematically developed only if its political system touches and mobilizes the masses of the people. I have investigated the case of Tunisia during the nineteen-sixties," he adds, "in order to illustrate and analyze this connection empirically."

There is a great need to establish empirically the connection between development and political mobilization, especially since the sympathetic attitude of development scholars toward this thesis of Mr. Rudebeck's has so far not produced the evidence necessary for us to accept or reject it. To show that political mobilization of the masses in one or two cases has been associated with political development is by no means a proof of the generalization as it has been stated by Mr. Rudebeck. Instances just as good could be cited in which development takes place in non-mobilization regimes, too. It is perhaps because the proposition in its present form is not subject to demonstration that Mr. Rudebeck does not substantiate it and devotes his efforts instead to the reconstruction of the process of party activity in Tunisia.

In 1961, the Tunisian government made the decision to take a more active part in directing the economy and christened its new policy with the name of "Neo-Destourian socialism." The event marks a major step in the gradual but decisive process of greater governmental controls in the manifold aspects of society. The author finds in this new policy the fledging of a mobilization regime under one charismatic leader with a mass political party in the leader's service. Of particular interest in Mr. Rudebeck's monitoring of this transition is his extensive and interesting account of the recruitment function of party leaders at the regional level. The reader will also find his analysis of the party relations to provincial government and the involvement of President Bourguiba's Destourian Socialist Party in the socio-economic spheres very enlightening.

Arstide Zolberg in his essay, *Creating Political Order: The Party States of West Africa,* has judiciously warned against making too much of the dichotomy between political party and state in the single party regimes. The evidence on party-state relationship in Tunisia provided by Mr. Rudebeck could just as easily be construed in favor of Zolberg's observation as of the author's contention that the party has supremacy over the official agencies of government. More attention should be given to the presentation and evidence for this thesis by Mr. Rudebeck before it can be confirmed.

The chapter on the labor movement and the Bourguiba regime is informative and interesting and clearly shows the low tolerance point of mobilization regimes for the autonomy of any countervailing political force in the system. Mr. Rudebeck deserves to be commended for paying some attention in his study to the attitudes of students, especially when we know that although students in Tunisia have not yet reached the boiling point in their protest movement like the Paris or the Columbia students, they have nevertheless been the most cantankerous of all the groups with whom President Bourguiba has had to grapple recently. The account is based on a sample survey of fifty-nine students in their first year at the College of Sciences of the University of Tunisia. The survey seeks to elicit some background information as well as attitudes on a variety of subjects ranging from socialism to opposition to the regime. The book concludes with a summary account on the six system functions, namely political socialization, recruitment, communication, articulation and aggregation of political interests, and the application of political decisions. However, the material presented in the book is not directly related to all these functions.

Only at the end, in the note on "Sources, References, and Literature," do we learn a little about the method Mr. Rudebeck followed in his study, and we may suggest that this question deserves a more complete and explicit account as well as a more prominent place in the book. It seems that the author leaned heavily on the interviewing technique and his ommission of the text of his various questionnaires is particularly regrettable in view of their great value in appreciating the results of his study.

Party and Politics is a useful book for students of North Africa and the developing countries and will certainly have its place in the literature.—Iliya F. Harik, *Indiana University.*

Government and Revolution in Vietnam. By Dennis J. Duncanson. (New York: Oxford University Press, 1968. Pp. 442. $9.50.)

Dennis J. Duncanson, now on the faculty of the London School of Economics, has impressive credentials for writing a political history of Vietnam. Following long service in the British administration of Malaya during the Communist insurgency, he served in the British Embassy in South Vietnam from 1965 to 1966, and during the four prior years, 1962 to 1966, he was a member of the British Advisory Mission to the Saigon government. (This is the second book of quality to come from the British Advisory Mission, which had six advisers at its peak. The first, *Defeating Communist Insurgency* [London, 1966], was written by its chief, Sir Robert Thompson.) In analyzing the political forces in Vietnamese society, Duncanson draws upon his professional service, which includes a number of Diem's monologues to him. However, this is not an anecdotal narrative but a carefully researched treatise in the spirit of an earlier book by another British scholar-diplomat, Donald Lancaster, whose *Emancipation of French Indochina* [London, 1961] was also published under the auspices of the Royal Institute of International Affairs. Duncanson reads Chinese, Vietnamese and French, and has documented his study from a wide range of sources. The book traces the

Chinese heritage in Vietnam, French colonial rule and its impact, the Viet Minh struggle, the regime of Ngo Dinh Diem and the role of the United States. Unfortunately, only part of the final chapter is devoted to the post-Diem years, a period for which analysis from the author's rich personal experience would have been welcome.

Duncanson selects themes from the past which help to explain Vietnam's troubled present. Whatever else divides the Vietnamese, he shows that they agree that their country must not be regarded as a dependency of China, but he adds the less widely recognized point that, historically, China has reached out to the south to influence Vietnam rather than to conquer it. In an excellent analysis, French colonial rule appears as a system which principally served French interests, though not exclusively metropolitan ones, and further increased the inherent Vietnamese political fragmentation. The French colonial regime is treated sympathetically, but the judgment is frank that it was a failure.

The author does not demonstrate the same emphathy for the Vietnamese Communists as he does for the French. He sees the National Liberation Front, and the Viet Minh before it, as simply the old Indochinese Communist Party which was a "subversive movement launched from outside Vietnam by the Soviet government as an instrument for embarrassing France." He regards Ho Chi Minh as an agent of anarchy who has worked assiduously to transform himself "from an obscure international agent of a foreign state" into a Vietnamese national leader. Though recognizing that the Communists have skillfully used persuasive techniques, he believes they have achieved their success primarily through coercion. They have exploited nationalism and social grievances in their scramble for power, but have little interest in the welfare of the Vietnamese people.

The author's diligent scholarship and elegant argumentation is admirable and yet his interpretation of the Vietnamese Communist movement and of Vietnamese nationalism is fundamentally weak. In this reviewer's opinion, he errs in viewing Vietnamese nationalism as "entirely negative' and "an excuse for sectarian bids for power." Nationalism has been a continuous driving force in the two post-World War II Vietnamese revolutions, and the Viet Minh and Viet Cong leadership have not simply manipulated nationalist slogans to install a foreign-oriented Communist regime, but fight as dedicated patriots—at great personal sacrifice—to achieve what they regard as independence and justice for their country. One need not find attractive the system they would implant, if victorious, to recognize that their strength has been in welding communism to nationalism as a single, powerful force.

Duncanson has captured the autocratic, arrogant style of the ruling Ngo family whose recipe for power he deftly describes as a "judicious mixing of intrigue, nepotism, simony, public humbug and private intimidation." It is surprising, therefore, that he attributes the collapse of the Diem regime not to its fundamental political weakness, which clearly emerges in his book, but to Diem's "ignorance how to administer the ordinary machinery of government." Administrative inadequacy of the Diem regime is his fundamental theme, and the American lack of understanding of this factor, his major reason for their failure in Vietnam. Is the British colonial administrator's "build a better administration" the magic counterpart to the American's "build a better mousetrap"? Despite this overemphasis upon the importance of effective administration, this book is unsurpassed in its description and analysis of modern Vietnamese administrative and legal practices. It is also excellent in describing the weaknesses of the American mission to Vietnam, particularly its lack of direction and coordination, the internecine rivalry between the State Department, the CIA and the military, competition which Diem, Nhu and their minions often exploited for their own interests.

As a valuable history of Vietnam, this book will remain on the reference shelves along with Buttinger, Devillers, Fall, Hammer and Lancaster, long after many of the current polemics have been forgotten.—Joseph J. Zasloff, *University of Pittsburgh.*

Communist China: The Domestic Scene 1949–1967. By Peter S. H. Tang and Joan M. Maloney. (South Orange, New Jersey: Seton Hall University Press, 1968. Pp. 606. $10.00.)

Professor Tang is a frequent contributor to the growing body of literature on contemporary Chinese politics. Of his previous publications the best known is a highly readable and useful text entitled *Communist China Today: Domestic and Foreign Policies* (1957 and 1961). The present volume, which he has written in collaboration with Dr. Maloney, constitutes another welcome contribution to the field. While drawing substantial material from the previous text, this book has incorporated sufficient new information to make its treatment of Communist China's internal affairs comprehensive and up-to-date.

The main body of the volume consists of eleven chapters. The first two chapters trace the ideological and historical background of the Peking regime. Chapter III examines the Chinese Communist leadership with special focus on Mao Tse-tung and his thought. The subsequent chapters give a detailed and balanced description of the entire structure and operations of the Communist system in China, including Party, state, economic, military, social, and cultural organizations. Particular attention is given to the nature of the Great Proletarian Cultural Revolution and its impact on the various aspects of national life.

In preparing this book the authors have used as their chief sources the great variety of publications and official statements of the Chinese Communists. The significance of such sources is attributed to "the naked frankness of the Chinese Communists in identifying their objectives and aspirations" and "the remarkable consistency in their line of reasoning and between their words and their deeds." The authors express some reservations about the reliability of occasional eyewitness accounts by foreign visitors but make no comment on the value of refugee interviewing as a research tool for the understanding of Communist China.

The book, on the whole, reflects a high quality of scholarship. It is well researched and clearly written. It provides both factual detail and perceptive analysis. The inclusion of a selected bibliography and numerous charts, illustration, and tables further enhances the worth of this volume.

One of the highlights of the book is its coverage of the Cultural Revolution. Although the authors' account and interpretations are generally interesting and objective, there is room for disagreement. They are correct in picturing the Cultural Revolution as essentially an ideological struggle and a determined drive on the part of Mao to rectify the Party and transform human nature in conformity with his revolutionary line. Nevertheless, they tend to overestimate the strength of "the thought of Mao Tse-tung" and are too optimistic about the beneficial effect of the Cultural Revolution. It is true that through the great upheaval Mao has succeeded in generating considerable mass enthusiasm for the revolution and in cleansing the Party of a large number of "revisionists." These gains, however, have been secured at a staggering price, namely, the breakdown of authority and discipline and the spread of factional rivalry and strife. From a short-run perspective, Mao indeed can prevail over the opposition and prove once more the "infallibility" of his leadership. But in the long run "the thought of Mao Tse-tung" is likely to undergo modification as the Chinese Communist revolution, like other crusading movements, will have to come to terms with realities.

There are other arguable points in the book. For instance, this reviewer finds the following statement (p. 29) less than persuasive: "Having tested these principles [Marxist-Leninist principles as refined and applied by Mao Tse-tung] and found them adequate, both pragmatically and intellectually, the Chinese Communists believe in the absolute effectiveness of this ideology for all areas of national and international programs and policies." The fact that the debacle of the Great Leap and setbacks in foreign affairs have led to a shape division between Mao and many of his top associates does not seem to fully substantiate this generalization.

These occasional lapses should in no way distract from the value of Tang and Maloney's commendable work. To the general reader as well as the specialist, *Communist China: The Domestic Scene* is highly recommended.—SHAO-CHUAN LENG, *University of Virginia.*

The Russian Revolution of 1905: The Workers' Movement and the Formation of Bolshevism and Menshevism. BY SOLOMON SCHWARZ. (Chicago: University of Chicago Press, 1967. Pp. 361. $8.95.)

Mensheviks are really extraordinary people. In 1917, confronted by the proclamation of a "dictatorship of the proletariat," an act which they condoned although they disapproved, they tried vainly to ameliorate the situation with a socialist compromise. Crudely rebuffed, they retaliated with the only act of protest of which they were capable—they walked out of the hall, and for all practical purposes, out of Russian history. Trotsky, exulting in the triumph of the moment, thundered his famous taunt after them: "Go where you belong from now on—onto the garbage heap of history!" The Mensheviks have deserved far better than "the garbage heap of history." Moving from country to country, in a state of permanent exile, they have nursed no illusions about a triumphal return nor have they recruited any congressional lobbies on their behalf. A half-century later, the surviving remnant asks no more than to be permitted to record the history of their movement, perhaps to clarify thereby the history of the Russian revolutionary movement in its broadest sense.

The fact that Solomon Schwarz was himself a participant in the Revolution of 1905 resurrects the perennial problem of whether revolutionaries should write the history of their own revolutions. Whatever the merits of the negative argument, Schwarz has managed to turn his personal involvement into an asset. Although he is scarcely writing a memoir, he does convey the feeling and spirit of the participant, of the young Social Democrat whose heart later was given to the Mensheviks after the fact but who was then caught up by the more radical and revolutionary activities of the Bolsheviks, the so-called "generation of Lenin." Much of the drama of 1905 is already known: the miserable war with Japan, Bloody Sunday, the incredible obtuseness of Nicholas II, the granting of the October Manifesto, and finally the ebb of the Revolution. However, most western histories are histories of elites in which the revolutionaries usually appear only as a nameless and faceless mass.

Schwarz's effort seeks to tell how the revolutionaries saw the revolution—at least in St. Petersburg.

In its place in the History of Menshevism series, his volume does appropriately provide a thorough analysis of the role, and sometimes lack of a role, of Menshevism in 1905. At the same time, it is a history of socialism in 1905 with as many accolades to the Bolsheviks as to the Mensheviks. As Schwarz demonstrates from the record of his own activities in 1905, somehow the militant activist tactics of Bolshevism seemed ever so much more relevant to the situation of that year. Schwarz does take issue with Bolshevik historians for distorting both Bolshevik and Menshevik roles in 1905 and particularly feels compelled to reassert the Mensheviks' historic role in creating the Soviet in 1905.

The volume is by no means a literary gem—a flaw that cannot be ascribed to the translator. It is pedantic, at times overly tendentious, and certainly far too generous with long quotations which might well have been shortened, summarized, or even omitted. The research is extraordinarily thorough and the author's grasp of the sources unquestioned. Curiously, there is no bibliography—an astounding omission in a monograph published by a distinguished university press. Schwarz is of no help in explaining the rural violence—for that the scholar must still turn to G. T. Robinson's *Rural Russia under the Old Regime*—the ministerial crisis, or the ambiguous position of Russian liberals. Schwarz's concern is with the socialists, a concern hitherto abandoned to Soviet historians who are saddled with their own albatrosses. Defects notwithstanding, by providing this intelligent analysis of socialism in 1905, Schwarz has filled a major gap in the history of the Russian revolutionary movement.—WARREN LERNER, *Duke University*.

Lesotho, Botswana, & Swaziland: The Former High Commission Territories in Southern Africa. BY RICHARD P. STEVENS, WITH THREE CHAPTERS BY H. GEORGE HENRY. (New York: Frederick A. Praeger, 1967. Pp. 294. $7.50.)

A decade ago news stories on Vietnam rated no more than page 37 of the *New York Times* under a Macy's ad; today there is similar unawareness, and similar future headline potential, of the new nations of Lesotho, Botswanna, and Swaziland. Tucked away in southern Africa, these former British High Commission Territories are shown on most atlases and globes under their previous names of Basutoland, Bechuanaland, and Swaziland (only the latter name has not changed, but it did receive its independence after the book under review was published). These countries and the even newer named Namibia (formerly South West Africa) are pawns in a fascinating and desperate gambit by South Africa to insulate itself against attack by the Black African states to the north.

The White minority rulers of South Africa cast their shadow over all these countries. Determined not to desegregate their own country out of fear that even a taste of the "good life" will start South Africa down the "slippery slope" of massive revolution, they intend to use the disenfranchised Black majorities as buffer units against the anti-White nations that dominate the African continent. Internally in South Africa, this program is taking the form of "Bantustans"—quasi-autonomous Black enclaves forming worker pools for "border industries" located in the White areas surrounding the Bantustans. Externally, South Africa hopes that Lesotho, Botswana, and Swaziland will become *de facto* Bantustans. The delicate policy appears to be to keep these Bantustans from becoming politically or economically independent, but also to give them enough of a stake in their own growing, dependent economics that they would not become allied with military or sabotage movements from the north. To do this, South Africa has to extend some economic aid, primarily infrastructural, to these enclaves so that they will have a degree of stability, be able to be easily policed, and become efficient sources of daily migratory labor. So far, South Africa's economic overtures and political gestures appear to have achieved some success, inasmuch as there seems to be no overt hostility to South Africa in these territories. Thus they may be on their way toward becoming buffer states in the true sense of that word. On the other hand, quite out of control of South Africa, guerrilla movements and hopeless but harassing border attacks against South Africa and Rhodesia are occurring with increasing frequency.

Against this strategic backdrop is an exceptionally fertile field for scientific political analysis. Are the new Bantustans viable new national units? What degree of integration are they achieving internally? What degree of integration may be found in the entire complex of nations in southern Africa? Is there a rudimentary "common market," as South Africa would desire? How many international "transactions" can be discerned, indicating the degree of reliance of the new nations on South Africa's economy? What is the proportion of imports to GNP? How are the guerrilla movements financed and supported? What is their local, as well as external symbolic significance? How do the United Nations' sanctions against Rhodesia influence the position of the former High Commission territories, and Namibia, against both Rhodesia and South Africa? These are questions not only of the greatest theoretical importance, but of potentially the greatest practical importance as well. By utilizing recent scientific and hard-data techniques developed in the discipline, political scientists are presented with a unique opportunity to apply their theories and methods to an area that will surely become the focus of world attention.

It is extremely ironic, therefore, that hardly any one at present seems to be taking up the challenge. Certainly the book under review does not even make a nod in these directions. There is no work, to the present reviewer's knowledge, dealing with integration and transactional analysis of the southern African countries. African Study centers seem to exclude this entire area from their main concern. Yet it is here where these centers, drawing upon interdisciplinary methods in political science, sociology, linguistics, and anthropology, among others, can apply their theories in a practical crucible. For example, the problems of political culture, nationalism or integration, linguistics (South Africa's new policy of instructing Blacks in their prescientific Native languages), political development, and so forth, here acquire an immense practical importance in an area that is soon destined for radical change.

The most that can be said for the book under review is that it has chosen an important topic. Otherwise, it is simply a "current history" approach to political science, highlighting willy-nilly those topics that appear in local newspapers and government reports—party histories, political personalities, names, dates, legal movements, economic plans, inventories of natural resources. Missing are the more significant factors of integration and nationalism, guerrilla movements, South African strategy, the policies of Zambia and Rhodesia, the Bantustan concept, transactional economic analysis, and related topics. To an extent, of course, the authors could make the hoary rejoinder that they did not choose to write about these things, and that they cannot be criticized for not writing a different book. However, it might be said by way of reply that they raise expectations by their title that are not fulfilled. And perhaps more importantly, their own conclusions are not proven by the evidence they adduce. Professor Stevens' most important conclusion is that the United Nations and Great Britain should furnish considerable economic aid to these territories so that "South Africa can be prevented from using these Territories as hostages in her conflict with the rest of Africa." But he has not shown what sense of the term "hostages" he means or whether any definition of such a term would be correct in his conclusion. Indeed, South Africa's policy might well be to use these lands as willing allies, and not at all as "hostages," and to this extent may itself be providing them with considerable economic aid. Perhaps by extending aid, the United Nations is simply lifting a burden from South Africa. Or perhaps not; maybe aid is desirable for other reasons. But to make a judgment on this issue, Professor Stevens would have to consider the problem from a much more complex strategic viewpoint than the recounting of facts, names and dates. His present work amounts to little more than three good encyclopedia articles under the headings of Lesotho, Botswana, and Swaziland.—ANTHONY A. D'AMATO, *Northwestern University.*

Die demokratische Willensbildung in den politischen Parteien. BY UTE MÜLLER. (Mainz: v. Hase & Koehler Verlag, 1967. Second edition. Pp. 175.)

The Basic Law of the Federal Republic of Germany of 1949 resolved that "the political parties participate in the forming of the political will of the people", that "they may be freely formed", that "their internal organization must conform to democratic principles", and that "they must publicly account for the sources of their funds" (Art. 21,1). The Basic Law further stipulated that "details will be regulated by Federal legislation" (21, 3). Eighteen years and some five abortive legislative proposals later, the *Bundestag* finally acted on this mandate and passed a "Law on the Political Parties" in the summer of 1967 (*Gesetz ueber die politischen Parteien* [*Parteiengesetz*] *vom 24. Juli 1967, Bundesgesetzblatt* I, 1967, No. 44 of July 27, 1967, pp. 773–781). After the processes of nominating candidates for public elections had previously been regulated by electoral legislation, and after the Federal Constitutional Court had already interpreted, and effectively applied (in the cases of the *SRP* and the *KPD*), the Basic Law's principles on the unconstitutionality of certain political parties, the debate preceding the legislation of 1967 had primarily focused on the following issues: 1) The publicity of the parties' revenues and financial transactions (cf., for an excellent recent survey, *Rupert Breitling's* article on "Literatur zur Partei- und Wahlfinanzierung", *Politische Vierteljahresschrift* 9 [1968], pp. 99–120, as well as the 1963 special issue of the *Journal of Politics*, edited by *Rose* and *Heidenheimer*); 2) Financial and other support from public sources for parties' election campaigns and related activities (*cf. Heinz-Christian Jülich, Chancengleichheit der Parteien,* Berlin 1967); 3) The internal organization of the political parties, especially with regard to the legitimization of party leadership and the democratic nature of their internal policy-making processes.

After *Ulrich Lohmar* (*Innerparteiliche Demokratie,* Stuttgart 1963) had provided an earlier study on the subject, *Ute Müller's* book is one of the last contributions to the debate on this third issue to have appeared prior to the *Parteiengesetz* of 1967. On the basis of the constitutional provision that the political parties' internal organization "must conform to democratic principles," she attempts to further interpret this norm, and undertakes to ascertain the degree to which the actual internal organization of the three major West German parties (*CDU/CSU, SPD,* and *FDP*) conform to that norm. The organization of the book reflects this approach: The first part provides an extensive account of the electoral and decision-making practices in the three parties against the background of their respective statutes; at the beginning of the second part, an attempt is made to translate the general constitutional principle of "internal party democracy" into some more specific normative categories which are then applied to the evidence provided in the first part.

It would appear that whatever value this study has for the political scientist, lies in the information contained in the first, descriptive part of the book. The second part, although interesting in its own right as a piece of legal-normative argumentation, is of less relevance to an understanding of the internal functioning of the German political parties. The first part, however, does present a few rather interesting insights into the internal workings of the three major parties—a field in which, given the general scarcity of information, every bit of evidence is highly welcome. With regard to the processes by which party offices are acquired, a rather striking general pattern, as well as some significant variations between the three parties, become apparent. The highest executive organ of each party on the national level, the *Bundesvorstand,* supposedly derives its legitimation from an election process in which the will of the party membership is expressed, even though this expression may be channeled through various levels of delegation from the local party unit on up. This "uninterrupted chain of delegation," however, is far from being a reality in all the parties, and is not more than an insignificant residual category in at least one. Thus, 77% of the members of the *SPD Bundesvorstand* are elected on the basis of such "continuous" delegation—still an impressive figure if compared to the 38% and 6% in the cases of the *FDP* and *CDU,* respectively. The remaining seats on each *Bundesvorstand* are filled in a variety of ways, primarily by cooperation, ex-officio arrangements, and the vote of non-elected party officials on lower organizational levels.

While some of these figures, given the complexity of the electoral and delegating processes, may be arguable within certain margins, the overall picture remains one of the existing party leadership exercising a virtually decisive control over its own perpetuation and replacement, with hardly any serious challenge from the party membership: "The incumbents of leadership roles are in a position to manipulate the electoral mechanism in such a way as to make their will the decisive factor in determining the future occupation of these roles." (p. 66). By and large, this pattern is replicated in the processes of internal policy-making, especially with regard to both the power and the representativity (or the lack of both) of the national conventions (*Bundesparteitage*) of the three parties. The analysis of the rules of procedure, and of the procedures themselves, tends to support, with minor variations across the parties, the author's summarizing statement that "the influence of the membership (on policy decisions) has to be regarded as very moderate, if not totally insignificant" (p. 92). Something of an exception in this regard is, again, the *SPD,* where a great deal of policy initiative is normally generated at the bottom of the party hierarchy but where, due to the traditional discipline of that party, not many of these initiatives are ever allowed to disturb the preconceived will of the party's leadership.

As a contribution to one narrowly defined aspect of the overall debate on the role of the political parties in the West German political system, *Ute Müller's* study provides some interesting data, and an intelligent, if at times less than cogent, normative evaluation of these data. One is struck, however, by the almost total absence of any conceptual framework which, especially in the first part, could have helped to provide some categories for analyzing what is now a rather overwhelming mass of minute detail. While this does not necessarily invalidate the factual information presented, it makes any attempt at explaining some of the more intriguing variations across parties and party levels virtually impossible. In this respect, *Lohmar's* earlier study on the same subject had shown a considerably higher degree of conceptual skill. It would also seem to be worth a critical note that not one non-German source is either used or even listed in the bibliography. Given the vast amount of research and theory on political parties and their internal operations available in such languages as English (*Lipset, McKenzie,* etc.), this selectivity, together with the total absence of any comparative consideration, gives the study a somewhat parochial quality. In terms of the scope of the study, it is regrettable that the author did not see the need for including a discussion of the role of the parliamentary parties (the *Fraktionen*) in the internal decision-making processes. At least for the parties in power, it should be quite obvious—as she herself quotes the then executive chairman of the CDU as admitting in 1962—that "the political influence of the federal party (in the past four years) was confined to approve of the decisions of the federal government and the *Bundestag* parliamentary party, and to accept these decisions as the party's program . . ." (p. 76).—HANS N. WEILER, *Stanford University.*

INTERNATIONAL POLITICS, LAW, AND ORGANIZATION

France, DeGaulle and Europe: The Policy of the Fourth and Fifth Republics Toward the Continent. By Simon Serfaty. (Baltimore: The Johns Hopkins Press, 1968. Pp. 176. $6.95.)

In a lucid and carefully documented examination of France's European and Atlantic policies, Professor Serfaty finds more similarities than differences between the Fourth and Fifth Republics, even though he characterizes the former as France "declining" and the latter as France "ascending." In the Atlantic Alliance, the leader of Fourth Republic France saw "a means of recovering, at least cost, Great Power status." (p. 40) As in the Fifth Republic, however, the fear existed that the United States was creating a "quasi-permanent system of intervention into European affairs." (p. 40) Between the Fourth and Fifth Republics, French attitudes about the Atlantic Alliance as the "best of the worst systems" did not change greatly. In the Fourth Republic the debate over the Atlantic Alliance had two focal points: Would the Alliance strengthen France and Western Europe? Would it contribute to a reduction in the East-West conflict? In the Fifth Republic, it might be added, French policymakers asked similar questions but reached different conclusions about the efficacy of NATO membership. In both the Fourth and Fifth Republics French leaders strove to restore France to Great Power status. If the preservation of the French presence in Southeast Asia and North Africa occupied the attention of Fourth Republic leaders, President DeGaulle saw the strengthening of France's position in Europe as the key to French influence in international affairs.

The parallels between Fourth and Fifth Republic policy extend to military affairs. Although the author might have addressed himself more fully to the evolution of French strategic thought, he nevertheless reminds the reader that France in the Fourth Republic, as in later years, was not prepared to provide large-scale conventional forces to NATO. If she left her nuclear defense to the United States, France nevertheless began in the Fourth Republic the development of a national nuclear force—a force which Professor Serfaty believes is likely to become obsolescent if the superpowers press their existing technological advantage. The author bases this judgement on the assumption that one or the other, or both, superpowers will develop ABM systems. He might have strengthened his case by reference as well to the offensive strategic capabilities, including multimegaton missile forces, which the superpowers will be able to deploy within the next several years. As a result, the imbalance between French strategic capabilities, on the one hand, and those of the United States and the Soviet Union, on the other, may widen.

The theme of continuity is present in Professor Serfaty's treatment of French policy and the unification of Europe. In the Fourth Republic French leaders expressed exasperation that Britain remained aloof from the European unification movement and showed a preference for closer ties with the United States than the Continent. In this reviewer's opinion, Professor Serfaty is correct in his conclusion that only a wholehearted British commitment to a supranational Europe could have led France to ratify the European Defense Community Treaty. In 1954, however, France was not prepared to be "shut up in a small, dark box" with Germany; by January, 1963, when DeGaulle both excluded Britain from the Common Market and signed the Franco-German Treaty, France had recovered a measure of strength and self-confidence unimaginable just a decade earlier. The French action of January, 1963, represented a change in policy, although the reconciliation with Germany remained a priority objective of French leaders in both the Fourth and Fifth Republics. In the post-war period, French support for German participation in European integration "represented a regional adaptation of the reliance on international guarantees" of the interwar period, in which France had surrounded her potential enemy with bilateral agreements. (p. 11).

For a book devoted to French policy toward the European continent, comparatively little attention is given to French relations with the communist states of Eastern Europe. Although he describes DeGaulle's long-held belief in the unity of states on both sides of the Rhine, the Alps, and perhaps the Channel for the achievement of equilibrium and peace in Europe, Professor Serfaty does not examine in detail the policies chosen by the Fifth Republic to hasten the creation of a Europe based on the Gaullist vision of "Europe from the Atlantic to the Urals." Given their importance in French policy especially between 1964 and 1968, the author might have enhanced the value of his work by addressing himself more fully to relations between France and the communist states of Europe.

Although much of the author's attention is focused on foreign policy, he examines changes in the French political system between the Fourth and Fifth Republics. With the emergence of a "new sociological France" which is more immune to the ideological quarrels of the past, "France is developing large and loose political formations of the American type." (p. 40) To what extent, the author might have asked, are the quarrels of the past likely to be replaced by a new set of divisive issues which will produce changes such as those which shook the Fifth Republic in the Spring of 1968?

Professor Serfaty concludes that, in the Fourth Republic, it was difficult for a government to devise a coherent foreign policy, or, having done so, to insure its implementation. "With every government described as a caretaker government, and every year regarded as a year of transition, the Fourth Republic itself was a Republic of transition, and even the well-known continuity of the heads of personnel at the Quai d'Orsay could not prevent *immobilisme* from eating at the conduct of foreign affairs." (p. 84) The feature of French policy, which the author does not adequately explain, is its continuity despite basic differences between the institutions of the Fourth and Fifth Republics. But the continuity of French policy, even with institutional changes between the Fourth and Fifth Republics, gives added weight to Professor Serfaty's contention that the demise of DeGaulle is not likely to lead to a transformation of France's relations with other powers. If the author is correct, his book has considerable utility not only because of its insights into French affairs past, but because of the perspective it presents to the student and policy maker concerned with the likely shape of French policy over the next generation.—ROBERT L. PFALTZGRAFF, JR., *University of Pennsylvania.*

Can We Win in Vietnam? BY FRANK E. ARMBRUSTER, RAYMOND D. GASTIL, HERMAN KAHN, WILLIAM PFAFF AND EDMUND STILLMAN (New York: Frederick A. Praeger, 1968. Pp. 427. $7.95.)

The Hudson Institute has found an admirable way to combat two damaging, and often undeserved, public impressions about independent, non-profit defense research contractors: that they are monolithic, and that they are stooges for, or at least constrained to be uncritical of, a Service or Administration policy line. The solution here is to bind together the disparate views of individual members and exhibit the Hudson staff to the public in the process of arguing and in many cases disagreeing sharply with each other and with official U.S. Government positions.

This dialectical approach is, moreover, substantively rewarding when the subject matter is so complex and controversial as that of Vietnam policy. No one writer, even if he tries, is likely to give attention to more than a few of the different relevant points of view and paths of approach to these problems, and each of the five writers represented here adds significantly to the discussion, as does their direct confrontation in a roundtable symposium concluding the book.

In fact, the adversarial format of the book seems so effective a framework for explicating major issues underlying policy questions that one wishes its potential had been exploited somewhat more. The papers by Frank Armbruster and Raymond Gastil, on the one hand, presenting the case for pressing on toward past Administration objectives in Vietnam via a considerably modified military/police strategy, and the opposing papers by Edmund Stillman and William Pfaff, arguing for cutting losses, seem to have been written in isolation from each other and rarely meet head on. Instead of the comparison of evidence and cross-examination of opposing hypotheses that the book's structure seems to promise, one finds that quite different factors and issues are raised in each essay, and the various assertions slip past each other with only occasional challenge. This is compensated by Herman Kahn's three chapters, in which he goes beyond the written contributions of the other authors to summarize and compare their points of view and differences.

The discipline of this responsibility is obviously valuable to Kahn's own style of presentation, which throughout this book is sober, judicious, and lacking in flamboyance. (His concluding sentence—apparently written after the Tet offensive, while the book was conceived and mostly written before it—is understandably but unwontedly diffident: "It seems to me that despite the general feeling today that everything has been tried and nothing works, and that we are in a morass, we can in fact responsibly—with good faith and good heart—dedicate ourselves at the least to an intelligent temporizing action, and possibly to victory itself." As a response to the clarion question of the title, this sounds rather an uncertain trumpet.)

Three of the authors—Kahn, Armbruster and Gastil—are described as in "limited but reasonably broad agreement with many of the policies and objectives of the Johnson Administration" (though Gastil's discussion of Vietnamese realities leads him to such conditional and pessimistic conclusions that it is not clear how far he should really be linked to the other "war-winners.") Nevertheless, they take strong exception to many Administration positions and policies (including "attrition" theory and practice, indiscriminate use of firepower, escalation as it has been pursued, and the then-current bombing campaign in Tonkin) and propose recommendations, many of which, though familiar in classified policy debate (various experts on counterinsurgency have proposed them before in Vietnam), would represent radical, and most improbable, changes in current U.S. strategy, deployment and approach.

These measures would, I believe, be to the good. (They would still, I also believe, be far from adequate to "win"; throughout, the discussion takes inadequate account of the strengths of the Viet Cong, and underrates the vital importance of political aspects of the struggle.) But the very fact that they have never been implemented properly, or at all, is by now a historical datum whose true significance these three writers manage to ignore. To see its bearing mainly as they do—as positive token that "there is large room for improvement" in U.S.-GVN progress and performance (p. 307)—seems almost perverse. The assertion cited is most certainly, poignantly true in 1968. But it was equally true in 1958, and 1961, and 1965. And the impending "possibilities" for improvement looked equally "great" (to cite another of Kahn's premises for his conclusion that "we may yet win the war in Vietnam") in those years. What makes these perceptions appear clearly inadequate today as a basis for either prediction or policy—just as, we can now see, they were misleading then—is precisely that the room for improvement remains today just as large, and in just the same places, as it was ten years ago, with nearly all the prospective "possibilities" remaining just that.

To understand why we have not done better in Vietnam, and to appraise the prospects and likelihood of doing better in the future, it is by this time far from enough to say, "There are promising measures, some of which worked well, in other hands, against other opponents elsewhere, that have never been fairly tried by the U.S. in Vietnam." One must ask, and attempt to answer, just *why* they were never tried; what reason there is, now, to believe they are more likely to get a fair trial in the future; and if they do, how likely it is they will be fully effective against this enemy, in this country and time?

It is the events of the last seven years, and especially the last three, that have demonstrated the salience of these questions (and suggested discouraging answers); though not, it would seem, to Kahn and Armbruster, whose recommendations and judgments of feasibility seem scarcely different from what they might have been in 1961 or 1965 (when they would have looked more adequate to many readers, including this one). One wonders what, if anything, these writers have learned from the painful and frustrating experience of those years: specifically, about the characteristics of the U.S. as counterinsurgent (and more generally, as interventionist and as *learner*), the GNV as ally, the VC as opponent, and Vietnam as environment for a revolutionary conflict.

These are specifics that must underly judgments that would tell the President (more cogently than "we may yet win") *how he should bet:* the *odds*—on adoption of policies, on implementation, on VC (and GVN) counters, on effectiveness—the *costs,* the *time* required, the *risks.* Such matters are gravely neglected in the discussions of Kahn and Armbruster.

It is only Stillman and Pfaff, in fact, who take seriously the word "we" in addressing the title question. This focus—which seems to me crucial—plus their reading of experience both prior to 1961 and after, leads Stillman to conclude (p. 164) that our present choice is between "a painful ending of the war now and a disaster later." Pfaff's subsequent, brief chapter on possible settlements is the best discussion of realistic negotiating aims I have seen, highly to be recommended to the incoming President and his negotiating staff.

Their emphasis—peculiarly identified with writings by Stillman and Pfaff—on factors of culture, politics and history, and particularly on American limitations ("Of all nations," Stillman remarks on p. 350, later excepting Albania, "I think that the United States may be one of the least capable of intervening in such a situation successfully") reflects a point of view that may not be conclusive but is highly relevant, neglected, and valuable. If it had been more adequately represented and understood much earlier in our involvement in Southeast Asia, we might have avoided an American tragedy.—Daniel Ellsberg, *The Rand Corporation.*

Anglo-Soviet Relations, 1917–1921. Britain and the Russian Civil War, November 1918—February 1920. By Richard H. Ullman. (Princeton: Princeton University Press, 1968. Pp. 395. $10.00.)

In this volume, the second in a series of three, the author deals with a very important case study in the politics and strategy of intervention—the British involvement in the Russian "civil war." Although this subject has been discussed in works by George F. Kennan and John M. Thompson and others, the virtue of Professor Ullman's book is that it offers a highly detailed examination of British cabinet-level decision-making in an area in which it failed. The author has made a very real contribution in utilizing and in quoting extensively from a variety of unpublished official documents and private papers. These include cabinet minutes, Foreign Office and War Office records, letters, telegrams, and memoranda as well as the diaries of Field-Marshal Lord Ironside (Commander of Allied Forces at Archangel) and of Field-Marshal Sir Henry Wilson (Chief of the Imperial General Staff). The quantity of previously unpublished material cited alone makes this volume an important study.

There emerges a record of piecemeal decisions, fragmented policy-making and improvisation. There was much disagreement among British policy-makers and advisers and as a result conflicting policies were adopted—which prompted Sir Samuel Hoare to observe in the House of Commons: "We have gone on the principle of backing one horse one day and another horse another day. We have sent people to deal with the Bolshevists and other people to deal with the anti-Bolshevists." (p. 117). To judge by the record, every effort to arrive at a statement of policy merely glossed over problems and issues and gave the appearance of being a decision when in fact there was none. The purpose of the intervention had changed over time; originally it had been prompted by military considerations related to the war effort against the Central Powers. As the struggle between Bolsheviks and Whites got underway, various cabinet members and officials adopted different arguments to justify greater (or lesser) involvement. Since the largest force (two divisions) was sent to Azerbaidjan and Georgia, due largely to the presence of Turkish forces in the Caucasus, some held that area and the Caspian Sea to be vital to the defense of India and Persia. Others emphasized the moral responsibility not to desert those Russians who had been "loyal" to Britain in 1917–1918 and who were now opposing Lenin's regime. The fear of a possible Russo-German alliance prompted some, especially Churchill, to favor intervention on the unproved assumption that a "grateful" restored Russia would support Britain after the defeat of the Bolsheviks. Thus Churchill and Henry Wilson were unsympathetic to the claims of the non-Russian nationalities to self-determination while Lloyd George at times expressed support for their cause (although his military aid to Kolchak and Denikin belied this sympathy). Wilson opposed intervention on logistical grounds and contended that Britain's troops were needed elsewhere.

The British intervention was highly limited and conditional (although the conditions varied in the four areas—North Russia, the Baltic, South Russia and the Caucasus, and Siberia), but London could not obtain the support of its allies. The British, Americans, French, and Japanese were often at cross-purposes; thus the Japanese were little interested in the outcome of the Russian civil war and may not have objected to a weak Bolshevik regime in Moscow if it increased their own opportunities for gain; the French spoke much of anti-Bolshevism, but their intervention in the Ukrainian coastal region was brief, and when Lloyd George sought to promote a negotiated settlement (at Prinkipo) he was undercut by the French—and also by the Conservatives in the House of Commons whose support he needed. The British generally favored the independence of the Baltic States while the French did not; the latter favored a restored Russian Empire that would be "grateful" to France and opposed to Germany.

More fundamental in explaining British failure were *domestic considerations: industrial unrest,* the need for troops in Ireland, the opposition of Labour and of various liberal elements, the impossibility of using conscripted troops, and the high financial cost of intervention. As a result it is not surprising that Lloyd George pursued a policy of drift and deviousness and grasped at various straws.

Although Professor Ullman's treatment conveys the indecision, bickering, and disagreement among British officialdom, it can be said that Whitehall had the following options: (a) withdrawal (in fact, the decision to withdraw forces from North Russia where nearly all British casualties occurred, was taken in March 1919 but was not effected until the autumn); (b) offering military and financial aid to the anti-Bolsheviks (this was done on a selective basis) to "give them a chance" and to preserve an alternative to Lenin's regime; (c) full-scale concerted intervention as advocated by Churchill (who was isolated in the Cabinet) but impossible under the circumstances; (d) negotiation (as, unsuccessfully, at Prinkipo and following the Bullitt Mission or, successfully, in the prisoner exchange agreement concluded at Copenhagen); (e) trade arranged indirectly in an effort to influence Lenin's regime without granting *de facto* recognition. In the end, Lloyd George was to rely on (a) and (e) after (b) and (d) had failed and (c) had been rejected; (e) was to facilitate (d).

The effectiveness of any intervention tends to depend upon the use made of the indigenous population. The failure to raise a large Russian army in support of the government formed at Archangel did not augur well for the intervention. The inability to establish a unified command meant that military operations against Lenin's regime were never coordinated. The existence of four or more Russian anti-Communist regimes as well as those of the non-Russian peoples who had declared their independence (Finland, Ukraine, Poland, Armenia, Azerbaidjan, Georgia, Estonia, Latvia, and Lithuania) made if difficult to form a common front. Kolchak and Denikin, whom the British decided to support, did not make effective use of British aid and were old-style Russian imperialists of a kind not likely to inspire confidence. The author appropriately raises the question of the kind of Russian regime that would have been established had the Bolsheviks been defeated and asks whether the Allies would have been able to come to terms with it.

In Professor Ullman's view, Lloyd George and Woodrow Wilson were wrong in believing that if the Whites deserved to win they would attract mass support. He contends that the two leaders employed a "fallacious analogy—that a civil war was like an especially violent election from which the party that can attract the most popular support will emerge victorious" (p. 351). Instead, he argues that a civil war is often determined by "the ability to combine rewards and punishments in such a way as to mold together an efficient, disciplined fighting force."

Intervention is a painfully difficult undertaking in the best of circumstances and requires clearly defined objectives that are compatible with capabilities. Professor Ullman's valuable case study and his analytical treatment in chapter nine provide a useful guide to the pitfalls and the costly nature of this kind of enterprise.—JOHN S. RESHETAR, JR., *University of Washington.*

Legal Order in a Violent World. BY RICHARD A. FALK. (Princeton: Princeton University Press, 1968. Pp. 610. $15.00.)

Of Richard A. Falk, it can be said at the outset that his is one of the most original and subtle minds addressing the theory and practice of contemporary international law. For nearly a decade, he has been concerned with the appraisal of the persistent resort by states to force as an instrument of national policy in the face of the real threat of nuclear disaster. No captive of the empyrean approach, he seems at times to despair of the prospects for the emergence of an effective legal order in a world of rampant violence short of the coming of World War III which might serve as the catalyst for the general adoption of those international institutional changes which would lead to a centralized, hence supranational, control of the use of force. Yet, he can "acknowledge that there has been a gradual strengthening of moral consciousness and human compassion so as to make the reorganization of the political basis of human existence on a global scale a meaningful goal." (pp. x-xi) Disarmament at the national level and its concomitant, the guarantee of effective maintenance of international security by supranational institutions are his keys to a warless world.

In *Legal Order in a Violent World,* Professor Falk examines with sophisticated insight the international uses of violence today, the problems of legal regulation of human conflict in a decentralized social system, and suggests ways of achieving his dual objectives. The book consists of fifteen essays, written between 1960 and 1967. Ten of the essays have been previously published in scholarly journals or in other collections, and three received limited circulation in a series sponsored by the Center of International Studies at Princeton University. Although the essays have been revised with a view to their incorporation into the present volume, the author states that he has endeavored to retain the policy context in which each was originally presented, so the date of principal composition of each is indicated.

The book is divided into four sections. Part I, "A Statement of Outlook," contains two essays on the control of international violence by international law. As the author points out in a later essay, law should be seen "not as a fixed matrix, but as a dynamic dimension of social change," (p. 345), and in his opinion, international law can be used in this way to bring about the new international order which he envisages. The third essay is a critique of the McDougal-Feliciano position on the subject as presented in their study, *Law and Minimum World Public Order: The Legal Regulation of International Coercion* (New Haven, Yale University Press, 1961). Here, Professor Falk takes exception to their reliance upon "reasonableness" as a control in national decision-making and as "the basis of a progressive system of world order." (p. 96) In Part II, entitled "Civil Strife, Intervention, and Minor Coercion," the author examines the emergence of civil war as a substitute for or alternative to international war and the development of third party intervention into an "interventionary pattern of diplomacy." (p. 103) He analyzes United States policy and practice regarding intervention with specific reference to the Bay of Pigs and Stanleyville incidents. Two essays are devoted to the exposition and defense of his view that United States involvement in the Vietnam war is not legally justifiable. As the alternative to continuing third party intervention by states, he urges the expansion of United Nations Intervention in situations of internal conflict as illustrated, in his opinion, by the Congo operation in 1960. The final essay in this group treats the use of propaganda as a form of intervention and urges caution in attempts to control it by international fiat because propaganda has a "legislative role in international society generating pressure for social change." (p. 364)

Part III on "Nuclear Weapons" explores the function of nuclear weapons technology in international politics. In the first essay in this group, the author analyzes the *Shimoda Case,* the unsuccessful effort by five Japanese claimants to recover damages from the Japanese government for injuries allegedly sustained as a result of the atomic bombings of Hiroshima and Nagasaki. Although the Tokyo District Court found no grounds for recovery of damages, it did hold that the use of the atom bombs by the United States was violative of established international law. The following two essays appraise practical policy limitations on the use of such weapons and the desirability of states' subscribing to a "no-first-use" rule as a control on such weapons. Professor Falk sees the *Shimoda Case,* the 1961 General Assembly Resolution 1653 (XVI) condemning resort to nuclear weapons as violative of the Charter, and the development of national policy controls on their use as pointing to a growing international consensus looking to the outlawry of these weapons. The last section of the book is concerned with the problem of controlling international violence while the international community moves in the direction of total disarmament. The appendices contain the text of the 1966 memorandum by the Legal Adviser of the Department of State on "The Legality of United States Participation in the Defense of Viet-Nam," the 1963 outline of Basic Provisions of a Treaty on General and Complete Disarmament in a Peaceful World, and the text of General Assembly Resolution 1653 (XVI).

Inevitably, a certain unevenness appears in the book, given the period of time over which the several essays were written. As the author himself points out, his discussion of disarmament predicated on the bipolarism of 1962 is not entirely *apropos* of 1968's "polycentric interpretation of international conflict." (p. 440) Furthermore, although the various essays are broadly addressed to the same general subject matter, there is a disjointed quality about the book which suggests that it should have "collected essays" as its subtitle and that the reader should be prepared to peruse the contents selectively rather than *seriatim*. Some flaws in editing appear, *inter alia* the addition of a paragraph to the Legal Adiver's memorandum which apparently belongs to the draft disarmament treaty text (p. 558), the date of this memorandum, and the title of the Bull *Inter Caetera*.

These considerations aside, Professor Falk's critical examination of the vital issues presented in the several essays makes *Legal Order in a Violent World* a valuable contribution to the current literature on problems of peacemaking. Students of international law and international politics will be glad to have these provocative essays available in a readily accessible form.—ALONA E. EVANS, *Wellesley College.*

Megamurder. BY E. L. M. BURNS. (New York: Pantheon Books, 1967. Pp. 297. $6.95.)

Deadly Logic: The Theory of Nuclear Deterrence, BY PHILIP GREEN. (Columbus, Ohio: The Ohio State University Press, 1966. Pp. 361. $6.00.)

Megamurder and *Deadly Logic* have more in common than their alarmist and depressing titles. Their authors each argue that gigantic errors in ways of thinking about Western military policy need urgently to be corrected. In neither book do the civilian strategists who have dominated the public debate receive high marks. Both are ambiguous as to relationships between defects of theory and military policies which the authors evidently dislike. One is explicit in urging us to get on with the business of arms control; this is implicit in the argument of the other.

There are contrasts, too. General E. L. M. Burns is a senior Canadian military man with wide experience in two world wars, in international peace-keeping, and in advising his government on disarmament matters. Professor Philip Green is a young American political scientist who stands outside of the "establishment" in the national security affairs community and does not like what he thinks goes on inside. Burns' book is aimed at his fellow military men and especially those who do not share the general's zeal for getting war back onto the battlefield and for putting protection of the civilian population far above "victory" as a military objective. Green's book is aimed at fellow social scientists and warns against "the new intellectual imperialism" of the deterrence theorists. The general writes calmly—he does not use the word "megamurder" in his text until the last sentence of the last page—and traces the evolution of bombing policies of which he disapproves from Douhet to the present. The professor writes polemically and writes much more about what others have written than about what military men and politicians have done.

The military do not, in Burns' view, share the whole responsibility for strategies of victory by urban holocaust, as his treatment of the savage and senseless bombing of Dresden shows. That event he sees as only one more example of the propensity of protracted conflict to escalate. One need not accept the utility of analogies drawn from protracted general war for understanding the arms race of the 1960s to agree that a plateau in arms levels provides the occasion for more intensive efforts to avoid future holocaust by negotiated arms controls.

Philip Green is a self-appointed David out to slay the Goliath of deterrence theory. His criticisms of the Brodies, Kahns, Morgensterns, Schellings, Snyders and Wohlstetters of this world are many and various. Method, motive, basic data and policy relevance are in turn questioned. Professor Green has read widely and carefully, and his criticisms are often provocative. To this reviewer, however, they seem less effective in concert than they might have singly. The central criticism seems to be that the deterrence theorists exhibit a "mutually reinforcing bias and trained incapacity." Evidently, Green would have a smaller share of research on arms and arms control carried on inside the ivory towers of the Rand Corporation, the Hudson Institute and the contract-fed university research institutes.

The deterrence theorists' models, Green complains, are too simple and too static and their data too unreliable. The canons of scientific rigor do not, however, oblige a scholar to stand mute until his model is perfect and his data complete. If that were the requirement, even Mr. Green's model of the military-intellectual complex might fail to meet the test.—WILLIAM T. R. FOX, *Columbia University.*

Western European Perspectives on International Affairs: Public Opinion Studies and Evaluations. ED. BY RICHARD L. MERRITT AND DONALD J. PUCHALA. (New York: Frederick A. Praeger, 1968. Pp. 552. $17.50.)

This book is rather like a Volkswagen microbus: it has esthetic difficulties but it can (and should) be extremely useful. One of its most valuable aspects is that it serves as introduction and guide to a large body of data which is available for further research—the USIA surveys which have been conducted approximately once a year since 1952 in four or more West European countries. Now comprising more than sixty national samplings, this material, as the editors point out, has a large potential which is still untapped. Some of its features are truly exceptional: not only were the same questions asked in all of the European countries surveyed in a given year, but certain items were used repeatedly over a period of many years, giving a basis for comparisons over time, as well as across cultures. One can undertake cross-national analyses of public response to major international events such as the Hungarian revolt of 1956—and eventually, one hopes, the war in Viet Nam or the Czech revolt of 1968: One might investigate the impact of such events on the images of given foreign powers, or on domestic political party preferences. (It seems most unfortunate that, since 1965, the USIA survey program has been curtailed, and the availability of survey results made subject to unpredictable delays—apparently because of the misuse of USIA reports by partisan leaders to demonstrate the success or failure of given administrations. One might think it would be possible to release at least the raw data from recent surveys, if not the official reports based on them; as of the present writing, however, no surveys made since 1964 had been declassified. American comprehension of European public opinion has probably suffered as a result.)

Besides introducing this material, the authors demonstrate a number of ways in which it can be utilized effectively. It is here that the microbus image may strike one: a surprising amount of worthwhile material is included in the book, without much regard to the attainment of any graceful overall form. Utilization of the material is incomplete; indeed, it scarcely could be complete, even with several volumes as large as the present one. What we have is a sampling of analyses which happen to have been done, coordinated by no readily apparent program. This is understandable in what is frankly an exploratory work.

That which is included has considerable substantive interest: for example, an article by Ralph K. White, on the meaning of the terms "socialist" and "capitalist" to European publics (as contrasted with their connotations in America). Or a series of articles by Leo P. Crespi and by Gabriel A. Almond analyzing results of the U.S.-Soviet race for international prestige based on scientific accomplishments.

Puchala's chapter on factor analysis in international survey research is interesting both substantively and methodologically; the technique should be particularly useful in cross-cultural comparison of responses. Puchala argues convincingly that this method can be valuable in analyzing *why* mass publics take given positions on given issues, by delineating the broader images which appear to influence responses to a particular question. There are limits, however, to what we can conclude from an examination of factor loadings, and Puchala's enthusiasm sometimes carries him beyond them. For example, he implies that the West German image of the U.S. in 1954 was favorable, in part, *because* it was associated with the generally favorable image which Germans had of foreign countries. The factor analysis merely shows that there was an association among images of foreign things, without specifying any causal relationship. As we see elsewhere in the book, German feelings toward the U.S. were much more favorable than they were toward any other country: I suspect that, if anything, the process worked in the opposite direction from that which he indicates. Overall, however, this section displays a number of keen and stimulating insights.

Another especially interesting section is Merritt's chapter on "Visual Representation of Mutual Friendliness." Considerable creativity is apparent in both the technique of graphic presentation and in the author's fruitful use of a distinction between salience and friendliness in mass images of other nationalities. The lack of a theoretical framework is particularly regrettable here, however. Although much of the substantive interest in Merritt's discussion lies in conclusions which one is tempted to draw about the relationships between public attitudes and public policy, any such theory remains implicit in the treatment. A considerable degree of correspondence between the two seems to be present, but we are not even sure of what Merritt regards as input and what as output. A future edition of this work might enhance its effectiveness through an attempt to tie these intriguing threads together in a more explicit manner. Needless to say, this is setting Merritt a large task in addition to the one which he has already handled ably.

This volume is not only a set of readings in European public opinion, but a reference work as well—containing more than 350 pages of percentaged marginal frequency distributions. The topics include: perceptions of the Cold War and Coexistence, European integration, arms control and disarmament, the Space Race, German reunification, and many more. Leafing through these pages, a number of enduring patterns emerge. For example, we note that French opinion has consistently differed from that of her neighbors, in that a plurality (and often a majority) of the French public has favored having France take a middle position between the U.S. and U.S.S.R., siding with neither. This has held true since 1952, long before De Gaulle's second coming, and France's withdrawal from active participation in NATO.

Because the USIA data recommend themselves for secondary analysis in graduate seminars, this can also serve as a textbook: written on a level which presupposes no previous training in survey analysis, it provides a guide to the items which are available in given surveys, examples of how the material may be exploited and some discussion of specific techniques. (The data are available from the Roper Center, Williamstown, Massachusetts or the Inter-University Consortium for Political Research, Ann Arbor, Michigan. The sampling technique of the respective European survey organizations seems to have improved over time, with the data gathered since 1960 evidently more reliable than earlier material.)

What the book lacks in neatness of organization is more than compensated for by its abundant inventiveness—in material used, techniques applied and the interpretations offered. The editors have also made an important contribution in facilitating the further use of resources which should be of immediate value to political science research and teaching—and may well constitute a significant asset to future historians of West European politics.—Ronald Inglehart, *The University of Michgan.*

Law in the World Community. By C. Wilfred Jenks. (New York: David McKay Company, 1967. Pp. 164. $7.95.)

This is a collection of lectures, delivered between 1964 and 1966, to law faculties and international lawyers around the world. The author touched bases in almost all blocs and continents.

One should not underestimate Wilfred Jenks. He is a highly skilled legal craftsman and has performed signal service as a pioneer international civil servant. He has been with the International Labor Organization since 1931—chief of its legal staff since 1940, and for some years now its Deputy-Director General. When he writes in depth on practical legal problems, especially those concerning the law of international organizations, no one speaks out of greater experience or with more authority. Among his more recent works, *International Immunities* (1961), and *The Proper Law of International Organizations* (1962) may be cited as examples of that kind of work. The contribution presently reviewed is of a different kind. Like his earlier books entitled *Law, Freedom and Welfare* (1963) and *The Common Law of Mankind* (1958), this collection deals in broad generalities and is essentially an exhortation to his audiences to construct a logical and consistent rule of law in world affairs.

It is hard to believe that any useful purpose is served by this kind of book. Certainly fallacious is his initial assumption that all international lawyers are dedicated to the same objective—"a common interest in peace, freedom and welfare," he called it in Warsaw; in Tokyo he assumed a central purpose of international law and international lawyers to secure the freedom of man. "Do we believe in freedom? Do we believe in law? . . . If we believe in these things, and have the courage and resolve to translate our beliefs into effective action, all our problems are soluble." (p. 102)

The objections to these assumptions are obvious. Not only is he glossing over the existence of widely differing national objectives, but even within each nation, its international lawyers differ in their objectives as they differ in roles. The policy advisor, the governmental apologist, the lawyer for an interest group, the truly independent scholar, the advisor to an international organization—all of these will have their own individual objectives. Law is instrumental, more often than it is a goal in itself. Indeed conflict, more than harmony, characterizes the objectives of international lawyers.

This reviewer agrees with Jenks' view that "the place of law in the world community is not a technical problem of jurisprudence but a broad issue of public policy." More study of the function and uses of law in the process of making foreign policy, and of its relationship to other factors, psychological, economic, and social, seems needed. But to start such study with generalities and exhortation is to put the cart before the horse. It must begin with detailed analysis of official behavior, based on data compiled from specific cases, and with a special focus on the points at which legal considerations have potential policy relevance. Fortunately, increasing effort is now being focused on studies of this kind. The relationship between law and policy is certainly more complex than Jenks suggests. One conclusion to which studies of the legal factor in policy-making seem to lead is that, even in the United States, the possible relevance of international legal considerations (especially in regard to long-range consequences) has not always been presented in the course of policy-making. In at least one important area—the regulation of the use of force—we have witnessed a steady erosion of the post-war legal consensus, which belies Jenks' optimism, and which is probably traceable in part to a lack of effective presentation in the decision-making process of the complexities of this law-policy relationship.—EDWIN C. HOYT, *University of New Mexico.*

The Relevance of International Adjudication. BY MILTON KATZ. (Cambridge: Harvard University Press, 1968. pp. 161. $4.95.)

In this little book, Mr. Katz deals with two of the more elemental functions of law, which are to curb violence and to resolve controversies. It is clearly argued that the limits to effective legal action in disputes between nations are to be found in the nature of the law itself. The question of what makes a controversy justifiable is not only a matter of the competence of the court to adjudicate, but is dependent also upon what there is in the dispute that makes it amenable, or not amenable, to adjudication by any court. The analysis that follows is, therefore, governed by certain premises with regard to the meaning and compass of the law.

The author significantly points out that international law entered the twentieth century as a comparatively rudimentary system both with regard to its doctrine and its institutions, and that a mutation was necessary to give a new direction both to law and diplomacy. The result has been a multiplicity of international organizations, which are regarded by the author as "specimens" of the mutations. We are reminded, furthermore, that a very large proportion of these organizations are of relatively recent origin. More than four fifths of the basic constitutional documents for international organization in force in 1960, were barely twenty years old.

It is contended that these organizations, dealing as they do with all manner of problems, have contributed in some degree to the development of international law. The main problem with which the author is concerned, however, is the significance of the mutation with respect to the central purpose of international law—the peaceful and constructive resolution of international conflict.

With respect to settlement of Cold War disputes, the author finds that whatever the attempts to enlist the norms of international law in support of respective claims, adjudication has not been resorted to in an effort to settle the conflict nor any other means of peaceful settlement, such as arbitration or mediation. "In sum, in international Cold War disputes, the conditions for adjudication or quasi-adjudication at best lie at the far edge of the spectrum. Typically, they lie beyond the limits." (p. 39). The questions posed at this point are: to what extent the apparent irrelevance of adjudication and arbitration of Cold War issues is due to an absence of tribunals to determine and apply law; or if these exist, to a lack of adequate means by which such institutions may assert authority; or, if the means exist, from the failure of the tribunal to make use of the available means. Moreover, to what extent is the apparent irrelevance here due to some inadequacy in the content of international law?

In chapter four of this study we find a most interesting discussion of the now famous South West Africa Cases. It is the best short account of this *cause célèbre* that this reviewer has yet seen. The apparent contradiction between the Court's Judgment of December 21, 1962 and the Judgment of July 18, 1966 is here effectively handled. Special attention is given to the third of four preliminary objections advanced by South Africa, which insisted that the dispute was not one within the purview of Article 7 of the Mandate, or one in which the Court had competence on the merits. The International Court of Justice, however, found in 1962 that each applicant in the case had "a legal right or interest in the observance by the Mandatory of its obligation . . . toward inhabitants of the Mandated Territory." (pp. 99-100). But in the opinion supporting the 1966 judgment, the Court appeared to be drawing a distinction between *locus standi,* as such, and standing to receive judgment on the merits.

Judge Jessup, in a separate opinion, pointed out the "utter futility" of such a procedure in asking the question: "Why should any State institute any proceeding if it lacked standing to have judgment rendered in its favor, if it succeeded in establishing its legal or factual contentions on the merits?" (p. 101). In his analysis of the 1966 judgment, the author calls attention to the limits of adjudication and quasi-adjudication in the settlement of international disputes. He regards an understanding of the effective reach and the limits of adjudication as imperative to the best results.

The Court, in the South West Africa Cases, for fear of involvement in a morass of political questions, had abandoned the field to political organs. The result was first the General Assembly Resolution No. 2145 (XXI) of October 28, 1966, which terminated the Mandate of South Africa and placed the territory of South West Africa under the direct responsibility of the United Nations. This was further implemented by Assembly Resolution No. 2248 (S-V) of May 19, 1967.

The question of legal authority on the part of the Assembly in this matter is carefully considered by the author. In his analysis of the action of the Assembly regarding the South West African Question, he finds that "a foundation in law for the authority of the General Assembly to cancel the Mandate has not yet been established." (pp. 123–124). The author's conclusion is that the withdrawal by the International Court of Justice in this matter brought about a great deal of confusion, and that the General Assembly has done little to mitigate the confusion. (p. 127).

At the end of this little treatise, Mr. Katz turns to a speculative appraisal of what consequences might have followed from the South West Africa Judgment had the one-vote margin been on the other side. In dealing with political action, the author contends that it need not be of the kind that has marked the deliberations of the General Assembly and the Security Council in recent years. He ends his argument on the note that "There could be political action infused with . . . a sense of law." (p. 155).—OSCAR SVARLIEN, *The University of Florida.*

Peaceful Conflict. BY EDWARD BERNARD GLICK. (Harrisburg, Pennsylvania: Stackpole Books, 1967. Pp. 234. $6.50.)

In this rather optimistic assessment of "civic action" functions of armies in developing areas, Dr. Glick explores a topic that has considerable contemporary significance for students of modernization, arms control, and counter-insurgency. He performs a valuable service in documenting uses of the military other than conducting war or engaging in recurrent coups. Unfortunately his treatment is marred by a lack of theoretical sophistication and critical treatment of source material.

Dr. Glick's principal thesis is that armies can, and in some cases do, provide positive benefits in economic and social development. Assuming that conflict and hence armies are inevitable, he argues that policy-makers should give increased attention to civic action programs—such as road-construction and education—in an attempt to direct military resources along peaceful channels of social construction. More extensive use of the military for non-combative purposes, Dr. Glick maintains, can facilitate in turn progress toward effective arms control agreements.

While a number of scholars have demonstrated at least a marginal interest in the socio-economic role of the military in influencing political change, Dr. Glick progresses beyond most existing studies by casting the developmental functions of armies in a broad comparative mold. He draws on the frontier experience of the American army in the nineteenth century, civic action aspects of U.S. foreign military assistance programs, and the modernizing role of armies in Latin America, the Middle East, Africa, and Asia to demonstrate the diversity of non-military uses of the military. His approach is largely policy-oriented, utilizing historical and contemporary data to establish general criteria to evaluate the effectiveness of civic action efforts and suggest guidelines for their future implementation.

Having established a potentially challenging framework of investigation, Dr. Glick nevertheless disappoints the reader on several counts. One major shortcoming is that the very scope of the book, while useful in providing necessary comparative perspectives, invites superficiality. Since Dr. Glick devotes less than 150 pages of the volume's total length to his global survey of civic action programs in countries as divergent as Vietnam and Israel, his account can obviously only skim the surface of extremely complex phenomena. As a result much of the book is a mere listing of civic action operations with little analysis in depth.

Even more disturbing is Dr. Glick's tendency to rely heavily on official government spokesmen and publications for most of his primary data. While former War Minister General Alberto Ruiz Novoa can undoubtedly provide crucial insights into the impact of civic action activities in Colombia, for example, his conclusions (pp. 104–5) may not necessarily coincide with the actual results of their implementation. To the extent that Dr. Glick repeatedly accepts official sources at face value, his book suffers seriously from the absence of critical evaluation of specific civic action programs.

A third major weakness of *Peaceful Conflict* is the author's neglect of systematic theoretical generalizations. Dr. Glick makes a modest attempt at classification when he distinguishes between "systematic and continuous," "formal but idiosyncratic," "insurgency-generated," and "non-insurgency-oriented" civic action programs in his concluding chapter. But his "theory" of non-military uses of armies is limited to an implicit ideological assumption that the military should participate in economic and social development—in the absence of civilian capacity to provide all necessary conditions of development—because this is a useful means to help prevent Communist-led insurgency and maintain international stability. This omission of explicit theoretical considerations makes it difficult to relate the book's findings to existing scholarly literature.

The shortcomings of Dr. Glick's basically intuitive approach are clearly evidenced in his definition and use of the development concept. Viewing development primarily as increased economic productivity, an enhanced standard of living, and advances in levels of mass education, Dr. Glick appears to underestimate the importance of accompanying attitudes as a necessary additional dimension of modernization. While civic action programs can undoubtedly contribute to quantitative changes within the former categories, their ultimate effect on the course of development will depend to a considerable extent on the degree of commitment by indigenous elites to genuine socio-economic progress. Civic action programs may serve simply as political palliatives to shore up existing regimes. Or they may function as a positive reenforcement of directed social change. Dr. Glick does not disregard such considerations altogether, but he would have considerably strengthened his presentation had he incorporated a more rigorous theoretical analysis of these and other possible patterns of development.

Despite its defects *Peaceful Conflict* is an interesting and provocative book. If it stimulates policy-makers to greater awareness of the importance of civic action programs and scholars to more thorough comparative investigations of the military's role in developing societies, it will fulfill an eminently worthy purpose.—M. DONALD HANCOCK, *University of Texas at Austin.*

ANNOUNCEMENTS

The sixty-fifth Annual Meeting of the Association will be held September 2–6, 1969, at the Commodore Hotel, New York City.

ARTICLES ACCEPTED FOR FUTURE PUBLICATION*

June, 1969

F. M. Barnard, University of Saskatchewan, "Culture and Political Development: Herder's Insights"

Richard W. Boyd, Indiana University, "Presidential Elections: An Explanation of Voting Defection"

John F. Manley, University of Wisconsin, "Wilbur D. Mills: A Study in Congressional Influence"

Norman H. Nie, University of Chicago, G. Bingham Powell, University of California, Berkeley, and C. Kenneth Prewitt, University of Chicago, "Social Structure and Political Participation: Developmental Relationships, I"

Richard G. Niemi, University of Rochester, "Majority Decision-making with Imperfect Agreement on Norms"

Kenneth Prewitt, University of Chicago, and Heinz Eulau, Stanford University, "Political Matrix and Political Representation: Prolegomenon to a New Departure from an Old Problem"

Howard Rosenthal, Carnegie-Mellon University, "The Electoral Politics of Gaullists in the Fourth Republic: Ideology or Constituency Interest?"

Bruce M. Russett, Yale University, "Who Pays for Defense?"

Giovanni Sartori, University of Florence, "Politics, Ideology, and Belief Systems"

Robert A. Schoenberger, University of Michigan, "Campaign Strategy and Party Loyalty: The Electoral Relevance of Candidate Decision-making in the 1964 Congressional Elections"

Henri Theil, University of Chicago, "The Desired Political Entropy"

September, 1969

Graham T. Allison, Harvard University, "Conceptual Models and the Cuban Missile Crisis"

Charles F. Cnudde and Donald J. McCrone, University of Wisconsin, "Party Competition and Welfare Policies in the United States"

Wayne A. Cornelius, Jr., Stanford University, "Urbanization as an Agent in Latin American Political Instability: The Case of Mexico"

Ralph M. Goldman, San Francisco State University, "A Transactional Theory of Political Integration and Arms Control"

Henry S. Kariel, University of Hawaii, "Expanding the Political Present"

Manfred Kochen, University of Michigan, and Karl W. Deutsch, Harvard University, "Toward a Rational Theory of Decentralization: Some Implications of a Mathematical Approach"

Richard M. Merlman, University of California, Los Angeles, "The Development of Political Ideology: A Framework for the Analysis of Political Socialization"

Norman H. Nie, University of Chicago, G. Bingham Powell, University of California, Berkeley, and C. Kenneth Prewitt, University of Chicago, "Social Structure and Political Participation: Developmental Relationships, II"

Nelson W. Polsby, University of California, Berkeley, Miriam Gallaher, Center for Advanced Study in the Behavioral Sciences, and Barry Spencer Rundquist, Stanford University, "The Growth of the Seniority System in the U.S. House of Representatives"

Ira Sharkansky, University of Wisconsin, and Richard I. Hofferbert, Cornell University, "Dimensions of State Politics, Economics, and Public Policy"

Jack L. Walker, University of Michigan, "The Adoption of Innovations by the American States"

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* Production exigencies may occasionally necessitate publication of articles in issues other than those given in this listing.

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COST:

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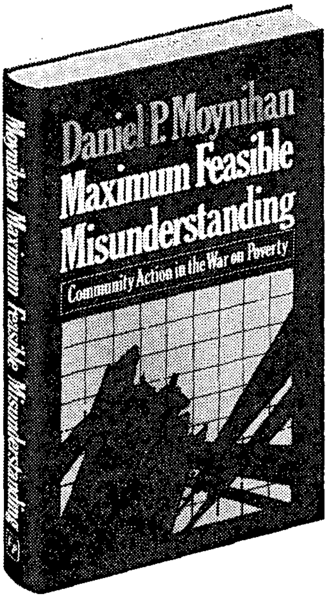
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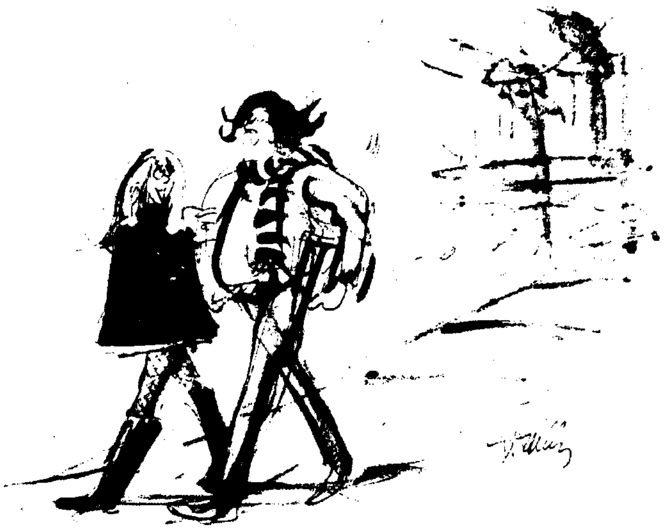

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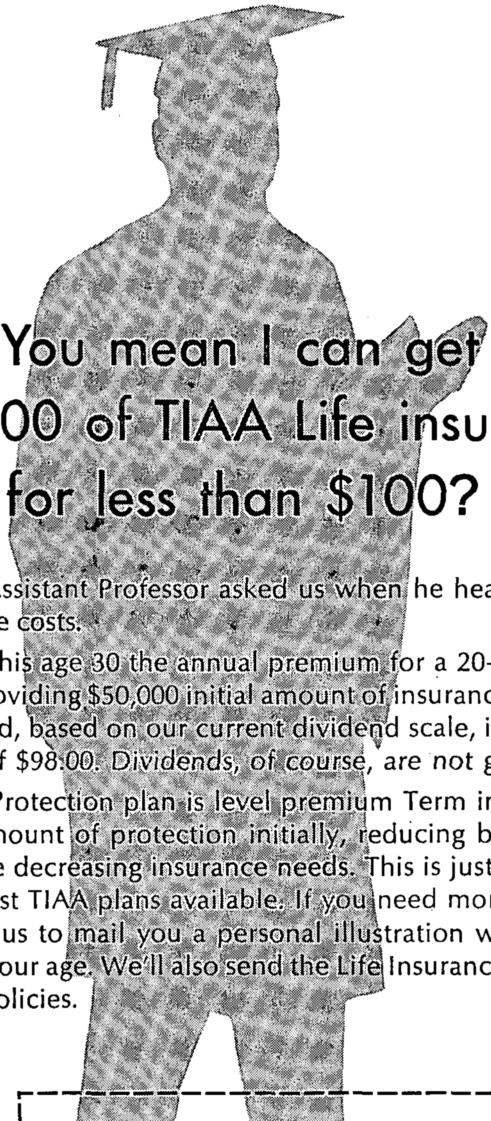

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